THE

MEDICAL REPOSITORY.

Vol. VI. NEW SERIES. No. II.

ORIGINAL ESSAYS.

An Account of the Endemic Yellow Fever, as it occurred in the City of New-York, during the Summer and Autumn of 1819. *By* CHARLES DRAKE, *M. D. Physician to the Bellevue Establishment.*

THE public are already furnished with two able and circumstantial relations of this endemic by Drs. Pascalis and Watts.* The design of their labours was to ascertain its origin, and the laws which regulate its propagation; without attempting to describe the disease, assign its type, or point out in what it differed from our autumnal endemics of former years. That they did not attempt to accomplish more, is probably to be ascribed to the fact that neither of those gentlemen attended a sufficient number of cases, to enable them to detail the general concourse of symptoms with that degree of minuteness, which is desirable in such cases. Indeed, the same remark will apply to most of the physicians of the city, none of whom attended more than four or five cases, and very few even that number. As physician of the Bellevue Establishment, the charge of the hospital opened for the reception of these

* See A Statement of the occurrences during a malignant yellow fever in the city of New-York, in the summer and autumnal months of 1819. By Felix Pascalis, M. D. and Part 2 of Vol. I. Hospital Register, by Drs. Watts, Mott, and Stevens.

fevers at Fort Stevens, near Hellgate, gave me a more ample opportunity of observing the disease, and noting its symptoms, than fell to the lot of any other practitioner; but at the same time I must confess, it was not so extensive as, under other circumstances, would be required of one who attempts to write its history; which I had not undertaken but from the failure of others, and a persuasion that the deductions I have made, from the observation of sixteen cases, will be found to apply with sufficient correctness to the fever as it prevailed generally.

The yellow fever had not made its appearance in the city of New-York since the year 1805. Nevertheless, the constituted authorities had not remitted the rigour of quarantine regulations, to guard against danger from abroad, nor relaxed the execution of those wholesome police arrangements which are calculated to obviate all sources of insalubrity at home. This long respite from an epidemic calamity began to be viewed by many as a pledge of total exemption; and measures, once deemed salutary, and submitted to with cheerfulness, were now felt to be prejudicial to commerce, and burdensome to the city. Fortunately for the cause of medical investigation, as well as humanity, these feelings had not been suffered to influence those who were charged with the execution of the health laws; for, on the appearance of fever in the latter part of August and first days of September, such had been the rigid execution of the quarantine system, that the Board of Health were led, as if by intuitive discernment, to the cause and origin of the disease, and the consequent adoption of measures, which, while they were successful in an unparalleled degree in securing the city from the ravages of pestilence, have presented in a clear, insulated, and conclusive manner, the causes which engender and propagate our autumnal endemics.

The weather, for several weeks preceding the appearance of this endemic, was remarkable for its unusual and continued heat, the prevalence of southerly winds, and

great drought. The same state of the air, and the same deficiency of rain, were felt, in a greater or less degree, throughout the Atlantic coast of the country, and appear to have had a very sensible influence on the diseases of the season: for besides the appearance of yellow fever in the cities of New-York, Philadelphia, Boston, Baltimore, Charleston, Savannah, and New-Orleans, the villages and many parts of the country, especially in the southern states, suffered considerably from obstinate remitting and intermitting fevers, dysenteries, &c.

In the month of August, the thermometer in the city of New-York was never at noon below 80° Farh. and during several days of that month it ranged beyond 90°. The atmosphere at the same time was uncommonly calm, and very little rain had fallen for several weeks, so that vegetation in the surrounding country, and as far north as the Highlands, was seriously injured; the crops of all kinds were light, in many places they totally failed, and trees, even in the lowest forests, perished from the extreme drought. Such was the scarcity of culinary vegetables, that the city was supplied from distant districts of the country, even from Albany;* a circumstance that had never before occurred.

The epidemics of 1798 and 1805 commenced at Old Slip, and the prevalence of 1819 was almost exclusively confined to a small district adjoining this commercial nuisance. Every case officially acknowledged to have been yellow fever, was satisfactorily traced to this spot, except two, and some doubts respecting these induced many to believe that these persons also received the disease by visiting the infected district. What are the circumstances, of this locality, it may be asked, to originate these pestilential fevers? Is it that this slip is appropriated to the use of vessels arriving from West-India ports, and other places within the tropics, where yellow fever prevails?

* Distant from New-York 150 miles.

So far from it, that it is almost exclusively occupied by coasters, a very small number of which come from ports south of Baltimore; and we must look to the actual condition of the slip and its vicinage for an explication of their rise and progress. This slip is very shallow, and one of the longest and most land-locked of any within the limits of the city, situated in the lowest and oldest part of the town, and further rendered liable to the accumulation of filth from an extensive sewer which empties at the head of it. Its filthy condition, and that of its immediate neighbourhood, at the time, was such as to attract the notice of the Grand Jury, who represented it as containing much putrescent matter, exposed on the recession of tide water to the influence of a hot sun, giving occasion to the emission of noxious and offensive effluvia. The houses on the west side of the slip, where a great part of the victims of the fever resided, are for the most part old wooden buildings in a decayed state, without yards, ill ventilated, and crowded with inhabitants. The offensive condition of the slip and sewer, during the hot days of the latter part of August, was also remarked by many citizens whose business led them to that part of the city. A respectable practitioner informed me, that about this time he was employed in an obstetric case, in the house occupied by Mrs. Kavenaugh, where herself and four others sickened with yellow fever, and that the house was much crowded, almost every room being occupied by a family, very filthy and confined. The room in which he was engaged was so offensive, that he was induced to throw open the window; but the wind blowing in a direction from the slip, he was assailed by such a blast of putrid effluvia, that he was constrained to close the window, and patiently endure the unpleasant smell within.

The best possible proof, that the fever originated from local causes, existing in and about this slip, the sewer, and the foul condition of many of the dwellings of the district, will be found by tracing the disease through its whole

course; for such were the active and efficient measures enforced by the Board of Health, that it was checked almost as soon as discovered to exist, and its entire history is destitute of all those adventitious and unimportant circumstances, which, in the accounts of similar endemics, have only served to confuse and complicate contested questions. As soon as a malignant fever was known to prevail in the district, the Board directed all vessels to be removed from the slip and adjoining wharves; the inhabitants were persuaded to desert the infected neighbourhood, which was estimated not to exceed thirty acres in extent, and was inclosed by a fence, to prevent useless visits: persons were employed to remove nuisances when practicable, or neutralize their deleterious agency by lime and other antiseptics; the poor were provided for at the public charge at Fort Richmond, on Staten Island; and a temporary hospital was opened at Fort Stevens for such of them as were attacked by the disease. To these measures are to be attributed the very limited number of its victims, as well as our ability to trace them to their origin. Of 63 cases, the aggregate number reported to the Board of Health, 34 were persons residing in the small block of buildings on the west side of the slip, noticed by the Grand Jury, which does not contain more than thirty houses, chiefly of wood, old and decayed; 9 in the block above, on the same side, and the remainder, except the two cases before mentioned, were persons who lived in different parts of the infected district, or whose occupations required them to spend much of their time there. The two exceptions may with much reason be referred to the same cause, as they resorted to a narrow, ill ventilated, and filthy lane, near the Fly Market; and from the strictest examination, it could not be ascertained that they had been for weeks previous within the interdicted limits.

The first case of the fever occurred in the latter part of August, in the building where four others sickened soon

after, situated in the block where the disease raged with the greatest violence. The month of September was the period of its greatest prevalence. Some few occurred on the 1st, 5th, and 6th days of October, and one as late as the 13th; after which the commencement of the cool season, and the appearance of frost, destroyed the cause of alarm, and the citizens, by the advice of the Board of Health, returned to their habitations.

Notwithstanding the clear connexion of cause and consequence, as exhibited in the above occurrences, and the entire view which the limited extent of the disease has enabled every one to take of all the facts in any way connected with it, in order to develop its origin, character, and mode of propagation; some have endeavoured to assign to it a foreign source, and have even in the first instance denied its identity with the yellow fever of former years, rather, it would seem, than admit that domestic causes could produce it.

In order to show on what probabilities the suspicions rest, which look to foreign causes for the source of this endemic, I will briefly notice the history of the vessels suspected, and which, in fact, were resorted to, to account for the appearance of fever in the city.

The French ship La Florestine from Martinique, and the brig Eliza from Charleston, S. C. have been designated as the vessels to which this imputation most strongly attached. The following extracts from the report of the Board of Health, dated Oct. 25th, 1819, must be deemed unexceptionable testimony; and while they prove that neither of them came to our wharves, they show also the very great difficulty of thoroughly purifying an infected vessel. "The French ship La Florestine arrived here in July, after a passage of 20 days from St. Pierres, Martinique. The yellow fever prevailed at that place at the time of her departure, and one of her passengers died with it there. She performed a quarantine of thirty days, underwent all the ordinary purifications, and on the 24th of

August the Health Officer reported that she was free from infection, and might safely be allowed to come to the wharves. The Board, however, ordered her to be anchored in the stream. In the beginning of September two of the seamen were taken ill with yellow fever, and she was sent back to the quarantine ground. From thence she went to sea, but having met with a storm, she put back in distress on the 25th of that month, when it was found that since her departure her captain had died of the same disorder."

"The brig Eliza arrived on the 13th August, after a passage of six days from Charleston, having on board a sick passenger, who died the same day with yellow fever. She performed a quarantine of thirty days, and was three times whitewashed, her limbers were taken out and cleansed, and she was otherwise purified, and on the 16th September she was allowed to anchor in the stream. On the 6th October her captain died of yellow fever, and on the 17th one of her seamen sickened with the same disease."*

On the failure of other means of introduction from abroad, the Baltimore packets, which usually lay at Old Slip, were alleged to have imported the contagious virus. Dr. Pascalis, in his account of the endemic before referred to, informs us, that on the 28th day of August, there was but one of these vessels lying there, which, when the slip was vacated by order of the Board of Health a week after, took her station at Pier No. 10, North River, where she remained till the middle of September, without leaving any trace of infection. The other Baltimore vessels had probably left that city before any yellow fever prevailed there.

Nearly a month after the subsidence of the fever, an agent of the Board of Health made a statement to that body, tending to show that it had been introduced

* Both these persons died at the quarantine ground. *Ed.*

after, situated in the block where the disease raged with the greatest violence. The month of September was the period of its greatest prevalence. Some few occurred on the 1st, 5th, and 6th days of October, and one as late as the 13th; after which the commencement of the cool season, and the appearance of frost, destroyed the cause of alarm, and the citizens, by the advice of the Board of Health, returned to their habitations.

Notwithstanding the clear connexion of cause and consequence. as exhibited in the above occurrences, and the entire view which the limited extent of the disease has enabled every one to take of all the facts in any way connected with it, in order to develop its origin, character, and mode of propagation; some have endeavoured to assign to it a foreign source, and have even in the first instance denied its identity with the yellow fever of former years, rather, it would seem, than admit that domestic causes could produce it.

In order to show on what probabilities the suspicions rest, which look to foreign causes for the source of this endemic, I will briefly notice the history of the vessels suspected, and which, in fact, were resorted to, to account for the appearance of fever in the city.

The French ship La Florestine from Martinique, and the brig Eliza from Charleston, S. C. have been designated as the vessels to which this imputation most strongly attached. The following extracts from the report of the Board of Health, dated Oct. 25th, 1819, must be deemed unexceptionable testimony; and while they prove that neither of them came to our wharves, they show also the very great difficulty of thoroughly purifying an infected vessel. "The French ship La Florestine arrived here in July, after a passage of 20 days from St. Pierres, Martinique. The yellow fever prevailed at that place at the time of her departure, and one of her passengers died with it there. She performed a quarantine of thirty days, underwent all the ordinary purifications, and on the 24th of

August the Health Officer reported that she was free from infection, and might safely be allowed to come to the wharves. The Board, however, ordered her to be anchored in the stream. In the beginning of September two of the seamen were taken ill with yellow fever, and she was sent back to the quarantine ground. From thence she went to sea, but having met with a storm, she put back in distress on the 25th of that month, when it was found that since her departure her captain had died of the same disorder."

"The brig Eliza arrived on the 13th August, after a passage of six days from Charleston, having on board a sick passenger, who died the same day with yellow fever. She performed a quarantine of thirty days, and was three times whitewashed, her limbers were taken out and cleansed, and she was otherwise purified, and on the 16th September she was allowed to anchor in the stream. On the 6th October her captain died of yellow fever, and on the 17th one of her seamen sickened with the same disease."*

On the failure of other means of introduction from abroad, the Baltimore packets, which usually lay at Old Slip, were alleged to have imported the contagious virus. Dr. Pascalis, in his account of the endemic before referred to, informs us, that on the 28th day of August, there was but one of these vessels lying there, which, when the slip was vacated by order of the Board of Health a week after, took her station at Pier No. 10, North River, where she remained till the middle of September, without leaving any trace of infection. The other Baltimore vessels had probably left that city before any yellow fever prevailed there.

Nearly a month after the subsidence of the fever, an agent of the Board of Health made a statement to that body, tending to show that it had been introduced

* Both these persons died at the quarantine ground. *Ed.*

by a vessel from Baltimore. Besides the very questionable character of a statement made so long after the occurrence, it has, I think, been fully invalidated by the inquiries of Dr. Pascalis, and the information afforded by the Assistant Health Officer;* whose situation enabled him to know more of the circumstances of the accused vessel, than could possibly be gathered from a creditable source by this agent. The statement alluded to, represents the sloop Hiram to have arrived at the quarantine ground from Baltimore, on the 2d of August, (having on the voyage lost a passenger, *supposed* to have died of yellow fever,) with three sick persons on board, who were *taken to the marine hospital;* where, it is further stated, one of them was admitted by some officer of the hospital to be a case of yellow fever. After some few days detention, this vessel was permitted to come up to the city, and take her station in the Old Slip. One of the passengers, Mary Stevens, took lodgings at Mrs. Kavenaugh's, the first victim of the fever, and assisted her, in connexion with Mrs. Brady, another victim, in washing the clothes of the crew, and also some of those of the person who had died at sea. This statement, so far as it differs from the official record of the vessel's history, rests on the parole evidence of the mate, and is proved to be incorrect: First—By the inquiries of Dr. Pascalis, who was informed, by a respectable person living in the same house with Mrs. Kavenaugh, that she never took in washing; that she had no convenience for such business, there being no yard to her house; and moreover, that no person by the name of Mary Stevens, or other person from Baltimore, lodged with her at that time. Secondly—By the statement of Dr. Harrison, who affirms, that the captain *testified upon oath*, "that at the time he left Baltimore, (July 2d,) no yellow fever prevailed there; that the person he lost on the passage, died in convulsions, in consequence, as he supposed, of

* Vide his Letter in Lang's Gazette, Nov. 20, 1819.

intemperance; and that the clothes he wore during his short illness, were thrown overboard." Of the three sick taken to the hospital from this vessel, one died of a bilious remittent fever, and *his* clothes did not go up to the city: a second had a mild remittent, the third an intermittent, and both recovered.

Of the 63 cases of yellow fever reported to the Board of Health, 37 ended fatally; 23 recovered, and the termination of the remaining three is unknown. Twelve of these cases were treated in the hospital at Fort Stevens, where four of them proved fatal. Besides these, there were several suspicious cases sent from different parts of the town, only one of which terminated unfavourably. Four of these were undoubtedly fevers of the same grade and character as those sent from the Old Slip, and I shall speak of them as such. They presented the same concourse of symptoms, as petechiæ, tenderness of the epigastrium, irritability of the stomach, dysenteric symptoms, with black and fetid alvine discharges, the same appearances of the skin, eyes, &c. Three of them were brought from the same house in Catherine-lane, and two of them had lately arrived from Ireland. This lane is very narrow and uncleanly, chiefly occupied by old wooden buildings, tenanted by the poorest class of Irish, where they reside in filth and misery. It could not be ascertained whence the fourth came. She was brought in a state of stupor and delirium, by two intoxicated men, who said she had been sick three or four days: this patient died on the third day after admission. Of 150 poor removed from the infected district of Old Slip to Fort Richmond on Staten Island, six soon after their arrival there sickened and died, without communicating the fever to any of the others.

Description of the Fever.—Usually the first intimation the person had of indisposition, was the sensation of extreme languor and deadly coldness, followed by slight irregular chills; sometimes this last symptom was entirely

wanting: violent pain of the head, chiefly super-orbital, with a dull haggard appearance of the eyes, sense of oppression and burning heat in the precordial region, rendered still more distressing by frequent retching, and vomiting, quickly succeeded. Sometimes the pain and tenderness extended over the whole abdominal region. The pain of the back, loins, and extremities, was excruciating, and frequently caused the sick to complain most of this symptom, and attribute all their sufferings to a cold and rheumatism. The bowels in the commencement were extremely torpid, and only partially evacuated by the usual doses of drastic purgatives. The pulse in every instance was small and frequent, as in enteritis, except that instead of being hard and corded, as in that disease, it was weak, and easily compressed. The eyes on the second or third day became suffused with blood, so as to make them resemble the common blood-shot eye; as the disease advanced, especially if the symptoms remitted, this blood was more or less absorbed, and left them tinged yellow. The skin was hot and dry, and on the superior parts of the body of a dirty dusky yellow, with some parts of a darker shade. The tongue was coated with a whitish or brown fur, and barely moist. The thirst was moderate, at least the sick did not frequently call for drinks; perhaps this arose from the fear of increasing the vomiting and gastric distress. The urine was small in quantity, and of a deeper yellow than natural. In the worst cases, a more aggravated and malignant train of symptoms appeared from the commencement; but in others they came on about the third, fourth, or fifth day, according to the intensity of the disease, and their supervention may be denominated the second stage. The first indication of their appearance was some disorder of the intellectual functions, at first occurring only at intervals, then becoming more constant, and assuming the character of delirium ferox, which usually terminated in stupor, from which the patient a few hours before death, especially on the appearance of black vomit,

or an analogous discharge from the bowels, usually recovered to a sense of surrounding objects and his own situation. The skin now took a more decided yellow, especially over the upper parts of the body, and became interspersed with numerous petechiæ, bright red in the milder cases, and purple in the more aggravated. If the irritability of the stomach had not been allayed, or recurred in this stage, the matter ejected was usually of a dirty colour, which, as the disease advanced, became of an intenser brown, resembling turbid small beer, or bloody mixed with a slimy fluid; and towards the close of the disease, it was sometimes the true black vomit, in which neither blood nor bile could be distinguished. The bowels, if they had not been freely evacuated in the first days, about the fourth or fifth, showed unequivocal marks of serious derangement. They were now moved by the mildest laxatives, disposed to frequent spontaneous discharges of black, fetid stools, which became mixed, as the disease advanced, with bloody, viscid matter, dejected with tenesmus and griping, till finally, just before death, they were very frequent, copious, and consisted chiefly of grumous blood. The urine continued small in quantity, and of a deep yellow, and was, in some instances, voided with pain, which may have arisen from strangury, caused by the succession of blisters. The oozing of blood from the nose, lips, gums, and tongue, which were now incrusted with a dry black sordes; the indistinct purple patches, or mahogany cast of the skin; the rapid, intermitting, and irregular pulse, with subsultus tendinum; the interrupted and laborious respiration; the diminished heat of the surface, with other symptoms of general collapse, were the usual precursors of death, which took place from the third to the ninth, most commonly on the sixth or eighth day.

The prognosis was favourable when the sick passed the sixth day, without the discharges from the primæ viæ showing any serious lesion of those organs, especially if the stools became bilious or natural, which evacuation appear-

ed to be more important and critical than any other. The first favourable indication was shown in the abatement, or diminution of the frontal pain, alleviation of the gastric distress, and the power of retaining food and medicine. A change of the colour of the skin, from the dull dark hue to the bright yellow of an ordinary remittent, was also very favourable, and accompanied by a return of the cuticular functions. The appearance of perspiration I viewed as no otherwise favourable, than as indicating a diminution of the irritability of the system, and a return of the different organs to their accustomed functions; for it was in vain to promote it, if the alvine evacuations did not at the same time become natural, and even freely produced; which appeared to constitute the appropriate solution of the disease.

The danger was to be estimated by the number and intensity of the symptoms which mark the second stage. Black vomit did not invariably attend the fatal cases: it occurred in only two of my patients, and about twelve hours before death. In a third, a bloody glareous fluid was thrown up just before that event. The dysenteric symptoms were much more constant; and, indeed, in every case, except two of the first sent to the hospital from Old Slip, and which presented the characters of an ordinary remittent, the alvine discharges were dark-coloured, very fetid, often bloody, and mixed with a viscid material resembling tar. Purple petechiæ and oozing of blood from the nose, lips, &c. were also very unfavourable. In one case, which ran its course with the greatest rapidity, terminating on the fourth day, the malignant symptoms, such as raving delirium, weak, intermitting pulse, disordered respiration, great distress and heat in the epigastric region, &c. appeared on the onset of the disease, confounding the stages, or rather only exhibiting the latter. Of the five cases which proved fatal in the hospital at Fort Stevens, two died on the 8th, one on the 4th, one on the 5th, and one on the 6th or 7th days. The convalescence

of the aggravated cases was remarkably tedious; there appearing to be a serious lesion of some important organ, from which the patients recovered with difficulty. One of the cases terminated in fatuity, attended with extreme dilatation of the pupils, a pulse which was thought to exceed 200, and a taciturn stupid state, resembling those cases induced by epilepsy. Two months after, the last time I saw the subject of this case, she continued in the same condition.

Treatment.—In two of the first cases, I made trial of the sub-muriate of mercury, with the view to excite ptyalism. In one of them, which was one of the mildest I treated, the gums became affected in 24 hours, and the patient soon after recovered. In the other, although there was ample opportunity for administering the medicine, as it was commenced early in the disease, and regularly continued in small and frequently repeated doses for five days before the appearance of the fatal symptoms, it produced no apparent effect. The alarming debility and the exhausted state of the vital powers, forbade the employment of the lancet. The signs of organic lesion of the stomach and bowels, as shown by the evacuations after the disease had continued four or five days, led me to make trial (for the removal of the irritation which existed in these organs, and to restore their suppressed excretory functions) of the liberal and daily employment of purgatives, conjoined with remedies that should, by their stimulant, sudorific, and nutritive properties, obviate the alarming debility, and support the vis vitæ. The almost uniform success which attended this plan of treatment, where the discharges had not shown serious morbid alterations, exceeded my expectations, and incline me to think that a constant attention from the commencement, to obtain free and repeated alvine evacuations, is the most efficient method of treating this disease.

The greater part of the sick had the bowels opened before their entry into the hospital, and on repeating the purgative, I did not find the torpor and inaction of this organ so great as other practitioners represented to have place in the commencement of the disease; for the usual dose of Ol. Ricini in general answered very well, and was repeated till the evacuations were free and copious. Afterwards the following laxative mixture was given daily to the extent of producing two or three alvine discharges;

R. Pulv. Rhei,
Carb. Potass. áá 3ss.
Ol. Anis. gutt. V.
Aq. Fontan. Oj. M.
Dos. ℥ij. quâque horâ pro re nata sumend.

In the interval of using the laxative, the patient took of the carbonate of soda gr. v. repeated every three hours, or the carbonate of ammonia to the same extent was substituted where the powers of the system were languid and sinking. At the same time the free use of warm wine whey, with the infusion of serpentaria, or eupatorium, as best agreed with the sick, was administered to keep up the tone of the system, and determine to the surface. The diet consisted of arrow-root with wine, or some other light nutritious preparation preferred by the patients. The irritability of the stomach, especially after the fever had existed a day or two, was best allayed by the use of Huxham's tincture of bark; which was also freely given on the accession of malignant symptoms, or alarming prostration of strength. The early and repeated application of blisters over the epigastric region, seemed to be of service in relieving the gastric heat and oppression, and applied to the extremities in the more advanced stage of the disease, were of signal service in maintaining the excitement.

The arrangements of the temporary hospital did not admit of autopsic examinations.

Observations on Ergot, by PIERRE CHATARD, *M. D. of Baltimore, in Reply to the Criticisms of* A. W. IVES, *M. D. which appeared in the last Number of this Journal.*

IN the last Number of the Medical Repository, Dr. A. W. Ives has furnished an article "On the Modus Operandi of Ergot in Parturition." The Doctor says, "I shall proceed to make some remarks on the mode of its operation; shall inquire what circumstances are most suitable to its use, and whether, independent of what may be denominated its specific properties, it possesses powers deleterious to the fœtus in utero." Had the Doctor performed the task he undertook, the obstetric art would have been infinitely indebted to him; but throughout his whole communication I see nothing but opinion, instead of proof: for example—to prove his first position, he says, "I believe, therefore, that the operation of the Ergot on the uterus is neither compound nor indirect, but that it excites the organ to action by a specific virtue, only known from its effects: as Ipecac. excites the action of the stomach, Jalap that of the intestines, and numerous diuretics the kidneys; and that from this simple view of the subject may readily be explained all the phenomena that have resulted from its use." This definition is obscure, and far from just: it is obscure, because the words, "neither compound nor indirect," convey no definite idea to the mind; and it is far from being exact, because there is no parity in the objects compared, ipecacuanha and jalap are applied immediately to the parts on which they are to act, whatever be their manner of acting; whilst diuretics and the Ergot, being applied to the intestinal canal, are at great distance from the kidneys and the womb. I do not, therefore, perceive that the Doctor's view of the subject is as simple as he would wish us to believe; and still less, that it affords an easy explanation of all the phenomena resulting from the employment of the *spurred rye*.

The rule that the Doctor gives for the proper administration of the Ergot, and at the same time to prove his second position, is not supported by a single case: it is a mere assertion, contradicted, in great part, by experience, and by the facts that I shall presently produce. He says, "It is only in protracted or tedious labours, and in such of those only as depend on the exhausted or inactive state of the uterus, that the Ergot *should* be administered." The rule is certainly good; but it is not exclusive. The complete dilatation of the orifice of the uterus, the partial action of that organ, as remarked by Dr. Ducachet, the favourable position of the child, and the good conformation of the mother, as I have already said, appear to me to be conditions more essential than the length of the labour or the exhausted state or inactivity of the womb, which are oftener imaginary than real. The womb, like all other muscular parts, may need some hours of rest, in order to recover its primitive state, that is, its strength, its energy, its sensibility, without there being, at the same time, any necessity of our stepping in to its assistance. Supposing then that the Ergot is administered in a case in which the womb is mostly fatigued, it is employed, not as an indispensable remedy, but simply to shorten the pain of child-bearing; and it is, above all, in this point of view, that it would be truly useful, if it could be administered without danger.

The Doctor is not more fortunate in the proof of his third position; for instead of showing that the spurred rye has no deleterious property, as he ought to have done, either by chymical means, or by solid reasoning, he is satisfied with saying, that the cases he criticises have convinced him, that, "though it sometimes destroys it, [the fœtus] it is not by poisoning, but by the mechanical action induced in the uterus." This surely is not sound medical logic. For, one death only by mechanical action, or by poison, should for ever proscribe a remedy, without which the obstetric art has no new danger to encounter.

By the pains he takes to show the active properties of the Ergot, it would appear that I doubted them! By no means: its effects are not doubtful to me; they are but too well demonstrated; but would to God that they had been demonstrated for the good of humanity, and not to satisfy human pride.

The desire for explaining every thing philosophically, and particularly the occult laws of nature, has been excessively prejudicial to the improvement of the healing art. It would be much more advantageous to study the effects of causes, than to seek to know those causes; and well has the Latin poet said, *felix qui potuit rerum cognoscere causas*. Causes are, in almost all cases, impenetrable; but effects are generally clear; and their utility may be perceived by the skilful observer, whose business it is to make the happiest use of them. When we seek truth, on any subject, we should seek it frankly, without troubling ourselves about the opinions of others, and endeavouring to bend fact to hypothesis: it is our duty to see and describe things as they are. This, I think, I have done.

Dr. Ives is a zealous partizan for the good effects of the Ergot, and by no means willing to allow it any bad ones, but such as result from the unskilfulness of the person employing it. "Incorrect theory," he says, "and injudicious practice, are unjustly bringing a valuable medicine into disrepute;" and therefore he endeavours to show that those properties which I have attributed to it, arise from the same cause. He, moreover, blames me for believing, with many other physicians, that the spurred rye acts by sympathy on the womb, and pretends that the consequences resulting from this opinion are of the most fatal kind. He thus expresses himself; "I am fully persuaded that this opinion of the action of the Ergot is exceedingly erroneous, and that the fatal consequences which have sometimes resulted from it, have been owing to these mistaken views of its modus operandi." The Doctor would have done us great service by ex-

plaining how this opinion can produce fatal consequences; but *he* understands the matter, and his word, of course, is sufficient. Nevertheless, I must confess, that being naturally somewhat incredulous, I do not believe that more harm can result from the employment of Ergot acting by *sympathy*, than if it acted *directly*, or in the manner understood by the Doctor. Whatever be the opinion entertained of its modus operandi, our only business is to employ it properly; and I anticipated the lesson which the Doctor gives in proof of his second position, when I said, that "if properly administered in cases free from mal-conformation of the mother or the child; where the constitution is weakened by former bleedings; or the uterus inert, requires some new power of action; then, I confess this agent may effectually impart a new vigour to the mother." I added, indeed, that it was to be regretted that we could not employ it without danger to the child, and that is precisely what has displeased the Doctor. If I err in attributing to it qualities injurious to the fœtus, the Doctor must agree that I have more reasons for believing so, than he has for denying it; for I have proved, and again prove my opinion by facts; (see Cases I. XIV. XVII. XIX. XX. XXI.) and the Doctor maintains his by a mere assertion, which is, that the death of the fœtus is produced "by the mechanical action induced in the uterus."

The Doctor himself affords me a proof in support of my opinion, when he says, "I recollect to have heard it asserted by one of the most distinguished physicians in our country, that in his own practice, it had proved fatal to no less than three children; and that he was determined never to use it again." With regard to the mechanical action of the uterus, which the Doctor substitutes for the deleterious one I ascribe to the Ergot, I shall in a moment furnish some substantial reasons for believing that the strong contraction of the uterus is but a very remote cause of the death of the child. Previously, however, to do-

ing this, the Doctor will permit me to strengthen my opinion by the authority of a physician who appears to be as prudent as himself, and to understand quite as well the circumstances most favourable to the employment of the Ergot; Dr. Ducachet, of New-York, whose talents equal his modesty, and who, to give me a better opinion of the new medicine, communicated to me the following case, in a letter, dated New-York, August 28, 1819.

" I was called, on the morning of August 24th, to a lady in labour with her fifth child. She is twenty years old, and of a full, gross habit. Her last child was very large, and died in the birth; but she has never had any difficulty in her former labours. Her waters were discharged on the first accession of the pains. She remained many hours in this condition, without any perceptible progress; indeed, it was not till late in the afternoon that I could discover the position. I bled her at noon, and gave her plenty of warm catnip tea, with the view of assisting the dilatation of the os uteri; but every thing was unavailing. Her pains continued to torment and exhaust her, but had no effect in advancing labour. I therefore composed them by a large anodyne, hoping that she would have a comfortable sleep, and afterwards more efficient pains. She rested for several hours, but when she awoke, was pretty much in the same situation;—the pains were precisely of the same character. She continued till next morning in this condition. The os tincæ was now completely dilated; but the head seemed to be immoveably fixed on the brim of the pelvis. I ordered stimulating injections to be given;—they quickened the pains, but did not advance the child. In the afternoon I concluded that there was no prospect of delivery by the unassisted efforts of nature. There were evidently four circumstances by which labour was rendered tedious:—1. The early rupture of the membranes: 2. An immensely large head: 3. General torpor of the system: 4. Imperfect action of the womb itself. The last

seemed to me the principal cause of the delay. I therefore determined to use the Ergot, and accordingly, at a quarter before five, gave a spoonful of the decoction, (i. e. ʒss. of the grains to ℥iv. of boiling water.) In ten minutes more I repeated the dose. Strong pains immediately came on, which continued, without cessation or abatement, until half past six o'clock, when the child was born. She was thus delivered in an hour and three quarters, after being in pain forty hours and a half. The child was living, and immensely large, weighing twelve pounds. The mother and the child are doing remarkably well.

"I think, as a general rule, that the os tincæ should be perfectly well dilated, before the Ergot is administered; and it is in cases of partial action of the womb, and insufficient pains, that it is particularly valuable."

This case, undoubtedly, speaks strongly in favour of the Ergot; and it is plain that the gentleman who employed it, knows perfectly well the circumstances in which it may be used with success. Now, if this same physician has reason to be dissatisfied with its use in subsequent cases, what objection can Doctor Ives have? The same gentleman writes to me thus, on the 12th September, 1820.

"I have had many opportunities of trying the Ergot since I last heard from you. I am fully and experimentally convinced of the accuracy of your observations. I have found out that this article will sometimes act upon the mother *alone*, and that it will sometimes produce injurious effects upon the *child*, without sensibly operating upon the mother. It is therefore a medicine, in the exhibition of which great discretion is necessary, and great prudence required. But it will sometimes effect wonders, and deserves a rank alongside of opium, mercury, and the other *heroic* medicines."

Shall I add to this, as collateral evidence of its deleterious qualities, the almost total disuse into which it has fallen in Europe, where it has been known and adminis-

tered to facilitate parturition for more than half a century? But Doctor Ives tells us, that all this is owing to the too strong and long-continued contraction of the uterus: if this cause produced such fatal effects in the child, there would be a far greater number of still-born children. In fact, do we not daily see children born with their heads larger by a fourth, than they are twenty-four hours after birth? How many of them remain in the passage two, four, and six hours, strongly pressed between the bones of the pelvis and the soft parts, without suffering the least inconvenience from it? I know that Doctor Ives will object to me, that this pressure is gradually produced in natural labour: but this objection is more specious than true; for I have often seen this pressure continued, and excessive for hours, without producing any ill effects. Cases VI. VII. IX. XXII. and XXV. would suffice to prove the improbability of this opinion, if the slightest reflexion upon the structure of the head of the fœtus, and in fact of its whole body, did not clearly show us the intention that nature had in providing against such unavoidable compression.

The fatal effects, then, which I attribute to the Ergot, are not produced by this compression. If the Ergot has no deleterious qualities, I should rather be disposed to attribute all these bad effects to a change in the quantity and quality of the blood of the fœtus. I will explain my meaning. Since it has not, to the present day, been proved, that the circulation of the blood is direct and reciprocal from the mother to the child, would it be unreasonable to admit, here, what has already been supposed, that the placenta performs the function of lungs to the child, and that the blood, which it receives from the mother, undergoes in it, a modification necessary to the life of the child, as that of the blood oxygenated in the lungs of the adult, is necessary to the existence of the latter? If, by the operation of any cause, the blood ceases to reach the lungs in sufficient

quantity, or in proper time, to be transmitted with the requisite qualities to the fœtus, it must, from that moment, be weakened, and begin to lose its vital faculties, in proportion to the action of the active qualities of the Ergot upon the nervous system, and physical force of the mother. Now, this is what may be inferred from observing the blood of the child, after cutting the cord. This blood is always black, and strongly carbonated, if the child had suffered for a long time. But, even according to this hypothesis, the Ergot would have fatal effects, which could not be attributed "to the mechanical action induced in the uterus," for the reason that we have given, and from the cases cited.

It is to be lamented that Dr. Ives, writing on the modus operandi of Ergot, has not informed us what *is* its modus operandi; that he has not explained to us whether this substance be tonic, emollient, or antispasmodic! and that he has not ascertained the doses suitable to each temperament, &c. &c. But I know his motive: the title of his communication was a mere pretext to enable him to criticise the cases of Doctor Chatard. Now, if the discussion which has resulted from this, be productive of the least advantage to the healing art, I shall thank Doctor Ives for having afforded me an opportunity of publishing, in a more ample and precise manner, my ideas on the subject of Ergot; for, being persuaded that truth, like a wedge driven but-end foremost, requires no little time to penetrate the human brain, I should have kept, for my own improvement, my two years further experience on this subject.

Let us now examine the *critique* of Doctor Ives in detail, and see whether he has been more fortunate by criticism than by comment. He begins by saying, "I shall briefly review the most important cases of Doctor Chatard, as they appear to me to be highly important, if not decisive, in solving this interesting inquiry. The limits of this communication will prevent my giving them in detail; but

it is intended to make such extracts as shall fairly represent them without injustice to the author."

Has Doctor Ives kept his promise? No: he has broken it in the very first *case*, from which he lops off the following words, which are absolutely essential: "The presentation of the head was natural; and nothing seemed to be objectionable against the trial of the spurred rye." If he had not lopped them off, he could not, without absurdity, have said, "From the first expression quoted, we are warranted in the belief, that the Ergot was given before the dilatation of the os uteri." A fine inference truly! What! because I admit (which by the way is not entirely correct) "that the Ergot is suitable and safe in every stage of labour, where the presentation is natural," it follows that the orifice of the womb was not dilated when I administered the Ergot? But do you speak in earnest, Doctor? Do you think that this consequence is a rigorous one?

But let us return to the misrepresentation of which I have so much right to complain: unless the Doctor did it unintentionally, I must believe that he preferred acting unjustly, to appearing guilty of an absurdity. In fact, since it is impossible to ascertain the good position of the head through the walls of the womb, it follows, that it must have been done by means of the dilatation of its orifice. The spurred rye, then, was not administered before the dilatation of the orifice, contrary to the assertion of the Doctor, who might, with equal propriety, have said, that I had given the Ergot because the woman was pregnant; for, how could I have decided that she was parturient, without having ascertained the greater or less dilatation of the orifice of the womb, produced at equal or unequal intervals by the contraction of that organ? The dilatation of its orifice, even to the size of half a dollar, is no criterion of labour, if there be not, at the same time, simultaneous contractions of the uterus; the pains which sometimes attend in these circumstances would only mislead the inexperienced accoucheur, who should undertake to decide

that labour was near; because, those pains might be intestinal. If then, more than dilatation and pain are necessary to a correct prognosis on this point, it is, *à fortiori*, necessary that the orifice of the womb be dilated, and sufficiently so, to permit the finger to pass through it, and examine a certain portion of the surface of the head or body of the fœtus, in order to ascertain its relative situation with regard to the pelvis, and thereby enable the accoucheur to form a positive and correct judgment. If the Doctor had said, that the waters not being discharged, I was unable to ascertain the exact position of the head, he would have spoken more reasonably: But, although it be difficult, in many similar cases, to discover the exact position of the head, still a sufficient degree of exactness may be attained to by a practitioner accustomed to such examinations.

From this detail, undoubtedly useless to the reader, but necessary to prove to the Doctor, that I reflect before I act, I hope that he will do me the favour to believe that I never act without knowing what I am about, as I have already said in the article which he criticises. It follows, then, that I was justified in using the Ergot after the lapse of twenty-four hours: the Doctor, therefore, is wrong in the very beginning, and particularly so, in believing that imperfect extracts can be given without injustice to the author.

The Doctor passes to the third case, and criticises it, by saying, "Whether, after copious bleedings, and the complete dilatation of the os uteri, the patient might not have been benefited by an ordinary dose of Ergot, it is impossible now to determine; but given in the quantity, and under the circumstances related, I conceive the author to be perfectly correct in supposing, that the case shows evidently the operative properties of the Ergot, that it provoked alarming convulsions, and produced dangerous consequences; and had he added the death of the child, I should have readily assented to his opinion."

If we are not permitted to try the good effects of the

Ergot, after the pains have lasted forty-eight hours; if we are to use it only in the case pointed out by Dr. Ives, or in similar cases to that of my ninth, which alone has escaped the severity of Doctor Ives, or in the case so well described by Doctor Ducachet, the remedy would, indeed, be of very little utility, because such cases are extremely rare, and the women, in general, have then more impatience than suffering. If the spurred rye cannot be useful in a case like this, in which the woman had been some time before sufficiently bled, to render a repetition of the operation unnecessary during labour, in which the strength had been worn out by pain and a protracted labour of forty-eight hours; in which the position of the child, and the conformation of the pelvis, warranted the most sanguine anticipations of success, then, I repeat it, we must give up the Ergot, if it be capable of occasioning the accidents which followed its use. With regard to the dose of the medicine, which meets the Doctor's disapprobation, I foresaw, at the time, the objection that might be urged against it, and showed that Doctor Foot had prescribed it, in doses sixteen times as strong. Doctor Ives has, therefore, treated me unfairly, and the Ergot was not, of course, unskilfully prescribed; and it has since been given with advantage in cases different from that to which he limits its use, as I prove, by Cases VIII. X. XVIII.

The fourth case which the Doctor criticises, so evidently demonstrates the bad effects of the spurred rye upon the mother, without injuring the child, that I am astonished that he did not pass it over. I assert, without fear of contradiction, that the Ergot was still more strongly indicated in this case than in the third; and I might add, that the Doctor's critique is not more surprising than was to me the bad effects of the Ergot in this case on the external parts of generation.

The sixth case is given without comment, and I cannot tell why, unless it was the Doctor's intention to show, that viewing the Ergot in the light of a tonic, and consequent-

ly as the very opposite of venesection, I did wrong to prescribe it here, because it was a case in which I said that bleeding appeared to me to be preferable. But I was authorized in thus acting, by the opinions of Doctors Stearns and Foot, cited by Doctor Ives, and with which I was as well acquainted as himself, before I ever employed the Ergot.

The seventh case, forsooth, operates strongly against me, because I was rash enough to give the Ergot, to ascertain its effect in dilating the orifice of the womb! Doctor Ives, therefore, does not fail to make the most of it, by saying; "But the Doctor, desirous of ascertaining the operative effects of a new remedy, seemed to disregard or neglect these considerations,"—that is, the exclusive rule which he has laid down for the employment of the Ergot. If I did not follow this rule, it is, because it was foreign to the aim and nature of my undertaking; nor in thus acting did I lose sight of the motive that guided me from beginning to end—the desire of discovering the truth, and of rendering myself useful; and I dare believe, that nobody but Doctor Ives will blame me for having so acted, particularly when the partisans of the Ergot have asserted so positively that its use is attended with no dangerous consequences.

As to the tenth case, which he merely mentions, and which he compares to the last, I there employed the Ergot, with a perfect knowledge of the habits and temperament of the woman, whom I had delivered several times before. It was impossible to meet with a case better calculated for ascertaining the prompt effects of the new remedy, and for satisfying my desire to know the truth with regard to it.

The twelfth case, which he likewise merely mentions, and which he also compares to the seventh, resembles it, positively, in no one particular; for I say,—"Much progress having been obtained," &c.; and I do not fear to assert, that it was still more favourable to the employment of the Ergot than the ninth, which the Doctor has exempt-

ed from his proscription. The Doctor, undoubtedly, mentioned this case inadvertently, for it proves strongly, very strongly, what the Doctor positively denies, to wit, "the direct and deadly influence on the life of the child," inasmuch as it produced no visible effect, either on the stomach or the womb, and the child was stillborn.

From all that has been said, therefore, I conclude, that Doctor Ives criticised Doctor Chatard's Cases without any benefit to the healing art, and that, of course, his criticism was useless.

A short time after the appearance of my communication in the Medical Repository, one of my friends, a distinguished physician in this city, told me, that he had no faith in the effects attributed to the spurred rye, and that all that I related of it, were but natural results, modified by circumstances. Another physician of the city, equally well known for his long experience, assured me that he had often employed the Ergot, and that he valued it no more than as so much saw-dust. The learned Professor of New-York, Dr. Samuel L. Mitchill, appears to me, likewise, not to esteem it very highly. All this, I confess, staggered me, and led me to imagine that I might have been deceived. In consequence of this I determined to continue my experiments. As it would occupy too much time and space to give them in detail, I shall merely give a summary, and draw the conclusions which naturally result from them.

Case I.

The woman who is the subject of this observation, being arrived at her full time, was five times seized with convulsions in the space of four hours, attended with continual delirium. A bleeding to the amount of twenty ounces, a cathartic, and sinapisms to the back of the neck, had produced no alteration at the expiration of eight hours. A second bleeding, to the amount of twenty-six ounces, sinapisms to the legs, and an antispasmodic draught, operated no other change in her deplorable situation, than in

weakening her a good deal, and contributing to the dilatation of the womb, which it rendered very soft. The delirium and convulsions remained the same during thirty hours. I took advantage of a slight pain to tear the membranes, and prescribed a drachm of pulverized Ergot in an ounce of hot water, to be taken by the teaspoonful every five minutes; the patient not being able to swallow more than this quantity at once. The convulsions ceased soon after; the pains were strong, and she was delivered in an hour and a half. The child was dead. Is the death in this case to be attributed to the Ergot, or to the convulsions? We know, by experience, that convulsions are not essentially fatal to the fœtus; and we know too that the rupture of the membranes has often stopped them by facilitating and accelerating labour. But, as in this case the action of the womb was developed in a rapid, strong, and continued manner, I do not hesitate to attribute the favourable change to the spurred rye; but, I dare not affirm that it was not, at the same time, fatal to the child.

It cannot be denied, I think, that the Ergot, in this case, acted directly on the womb; for, as it had no perceptible action on the stomach, it cannot be said to have operated by sympathy. If it should be objected, that the dose was too strong, my reply is, that the case was an urgent one, and required promptitude of action. But I have already given my excuse.

Case II.

The waters had been flowing from the subject of the present case twelve hours before the pains of labour commenced. After sixteen hours of suffering, without much advancement, I bled her copiously, to diminish the muscular power. The bleeding seemed to slacken the pains somewhat, but by no means contributed to impart vigour to the womb. The head was already considerably advanced in the inferior strait, and offered a good pre-

sentation. I gave thirty grains of powdered Ergot in a little warm coffee: the remedy acted in ten minutes, and the woman was delivered in forty-five. The child was not in the least affected by it. In half an hour after the mother was seized with a giddiness and head-ache, of which she was relieved after vomiting considerably.

Case III.

The lady who is the subject of the present case, was the mother of many children, and had always had tedious labours. She had been in labour more than twenty-four hours, when I broke the membranes to accelerate delivery, but without any good effect. The presentation being good, the orifice was much dilated, and the head considerably advanced into the lower basin; I administered a dose of powdered Ergot. It produced no effect either on the child or the mother. She was delivered two hours afterwards in the ordinary way, that is, by insensible degrees, and without much pain.

Case IV.

In this case, the lady had already had thirteen children; and a hemorrhagy, which had lasted fifteen days, had weakened her excessively. On examination, I found the orifice soft and dilated; I therefore took advantage of the pain to break the membranes, and enable the head to fix itself as much as possible in the orifice: this succeeded, and the hemorrhagy was stopped. But the labour continued slow; and fearing lest the lady should sink through extreme weakness, I administered two ten-grain doses of powdered Ergot in some broth, a quarter of an hour apart. The medicine acted, and the child was born in less than half an hour. The child was *asphyxiated;* and it was with great difficulty that it acquired the faculty of breathing. Its blood appeared absolutely carbonated. The mother experienced no effect from it.

Case V.

Lucy, a black woman, whom I had already delivered of

her first child, with success, by the forceps, after a painful labour of five days, sent for me to assist her in her second delivery, after having been in labour upwards of fifty hours. Every circumstance being favourable to the employment of the Ergot, which was particularly indicated by the extreme weakness of the woman, and the inactivity of the womb, I administered it in ten grain doses, at intervals of ten minutes. Its effects upon the womb were evident after the second dose, and the woman was delivered twenty-five minutes after the fourth. The child was born *asphyxiated*, and it was with difficulty that I could restore it. The Ergot produced no effect on the mother.

Case VI.

The woman, in the present case, was the mother of many children, and had had several abortions. She had already suffered excessively during twenty-four hours, and being alarmed at her situation, she sent for me. I found the orifice of the womb pretty well dilated; but the head, which apparently presented well, was still in the upper strait. Nevertheless, I ventured to give her half a cupful of an infusion of Ergot, made by pouring one of boiling water on a drachm of the drug. The remedy appearing to operate at the expiration of fifteen minutes, I gave the second dose with such effect, that the womb seemed to me to undergo incessant contractions during the space of fifty-five minutes, which procured a happy delivery both to the mother and the child. This case, besides proving the direct action of the Ergot upon the uterus, clearly shows that the mechanical action of that organ is not enough to produce the death of the child; because the compression was as strong as it could be, and lasted fifty-five minutes.

Case VII.

In this case the woman was in labour with her first child. A considerable deformity in the *vertebral* column

caused me some uneasiness as to the issue. I ascertained, however, after a careful examination, that the pelvis was not badly formed. The pains having been strong and continual during the period of thirty-six hours, without propelling the head, which presented very fairly, I administered an ounce of an infusion of two drachms of Ergot in three ounces of boiling water. This dose was ejected in twelve minutes, but nevertheless exhibited signs of having acted upon the womb, by the progress of the child. A dose similar to the first was now given, and the stomach retained it thirty-five minutes. Its effects were such, that the child's head advanced astonishingly. The remainder of the infusion was divided into two doses, which were given at intervals of twenty minutes, to keep up the action of the womb. But all its effects, which were great, were productive of no advantage; the child's head appeared to be immoveably fixed in the lower strait; the woman's strength was declining, and I had recourse to the forceps. In a few minutes I had the pleasure of accomplishing the delivery happily to mother and child. The spurred rye was in this case evidently useful, and did not occasion the death of the child, by acting with very great power on the womb, which would have followed it, if the child's death were produced "by the mechanical action induced in the uterus." It did not, it is true, prevent the necessity of using the forceps. But I do believe that we are to attribute to it the success with which the latter was used.

Case VIII.

The subject of the present case was bled after being thirteen hours in labour of her first child. The bleeding did not assist the progress of her labour, but it relieved the pain in her back, which was excessive. After the lapse of thirty-four hours, the contractions of the womb had been productive of no other effect than the complete dilatation of its orifice. The head remained very high up in the

pelvis, and there was no hope of a speedy delivery, when I determined to employ the Ergot in the manner above described. Two doses of it produced delivery in thirty-eight minutes, without affecting either mother or child.

Case IX.

The lady whose case is now cited, was the mother of six children. She has had pains during the whole day and the greatest part of the night. On examination, I discovered that the orifice of the womb was thin, very much dilated, and that to produce delivery, it was only necessary to impart to that organ the requisite energy, which appeared to be wanting. An infusion of Ergot, made by pouring three ounces of boiling water on two drachms of the drug, was administered. Two ounces given at once brought on delivery, by a single contraction, which lasted twenty minutes. It affected neither the mother nor the child, which last must have been excessively compressed during twenty minutes.

Case X.

In this case the woman had been in labour fifteen hours of her first child, and the pains had produced no effect, although the orifice of the womb was soft and very much dilated. The breech of the child presented well. Having ascertained that this woman, naturally weak and nervous, had been bled several times during her pregnancy, I employed an infusion of Ergot in the quantity of two drachms in three ounces of boiling water. I gave an ounce every ten minutes. The womb recovered its energy in half an hour, and at the expiration of forty minutes more, the woman was happily delivered of a fine child.

Case XI.

In this case the waters had been running off during three days, attended with slight pains. A violent head-ache, with fever, had rendered bleeding necessary, and the ex-

treme delicacy of her constitution led me to fear that violent and long-continued pains might be productive of fatal consequences: therefore, the orifice of the uterus being sufficiently dilated, and its action very weak, I gave four ounces of the infusion of the spurred rye, by table spoonfuls, in the space of half an hour; and in three quarters of an hour after, the woman was delivered of a small and extremely puny boy, who, nevertheless, was not affected, any more than the mother, by the use of the Ergot.

Case XII.

The mother of two children was attacked with a violent headach, after having been five hours in labour. The headach was relieved by bleeding; which did not, however, increase the action of the womb, whose weak, but repeated, efforts did not advance the labour. Six hours had expired since the bleeding: the orifice of the womb was dilated, and the head presented well. I now thought proper to use the Ergot, and two drachms of it were infused in three ounces of boiling water, a third part of which was administered every fifteen minutes. The woman was delivered in an hour and ten minutes after the last dose.

Case XIII.

The subject of the present case, at full time with her first child, had suffered eleven hours with inefficient pains, although the orifice of the womb was fully dilated, and the child's head presented well. The Ergot, prepared as above, was given every ten minutes, and brought on delivery, without accident, in an hour and twenty minutes.

Case XIV.

The woman, in the present case, was weak; and had suffered for more than twenty-four hours with pains, which at times were pretty strong. She had been bled several times during pregnancy. The orifice of the womb was very much dilated, and the head of the child was already

far advanced in the lower strait. The infusion of spurred rye was administered, and having produced no effect at the expiration of seven hours, I employed the forceps to procure delivery. The child, which was quickly extracted, was dead, and weighed ten pounds and a half. The death of this child, which was her first, could not be attributed to the forceps: it was easily seen that it had been dead some time.

Case XV.

The subject of the present case, in labour with her ninth child, suffered, as in former labours. during two days; and delivery appeared to be yet distant some hours, although the orifice of the womb was very much dilated, and the child's head presented well in the lower strait. The Ergot administered in infusion, to the amount of two drachms, was productive of no effect either on the womb or the child.

Case XVI.

The woman in the present case, being in labour for the fourth time, had experienced inefficient pains for about thirty hours. Every thing being favourable, I gave her half a drachm of powdered Ergot in a little warm tea. In twelve minutes the pains became stronger, and more frequent during thirteen minutes more, which brought on a happy delivery.

Case XVII.

In the present case, the woman was in labour for the first time. The waters began to pass off without pains; but these appeared soon after, and continued feebly twenty-nine hours, at which time the orifice of the womb was much dilated, but the head remained in the upper strait. Thirty grains of powdered Ergot, in a little cinnamon water, hastened the pains in twenty minutes; and the head, after having passed the upper strait, was placed under

the arch of the pubis, in the most favourable manner: it had nevertheless ceased, for a considerable time, to advance. A second dose of spurred rye was administered without any effect. The woman, having three hours afterward taken a cup of broth, vomited up all the Ergot contained in her stomach. From that moment the pains entirely ceased; and reappeared afterward at long intervals. Finally, at the expiration of forty hours, giving up all hopes of seeing the labour terminate without the assistance of art, I applied the forceps, and relieved the anxiety of the woman in a few minutes. The child was dead. It seems to have died of convulsions three hours before the use of the forceps.

Case XVIII.

In this case, the lady, pregnant of her first child, had for about twenty-four hours experienced pains, sometimes sufficiently strong, but inefficient. The head was still very high up in the pelvis, and the orifice of the uterus was not much dilated. She had been bled recently, and once about the middle of her pregnancy. Being naturally weak, and her constitution delicate, I thought it expedient, in these circumstances, to administer twenty-four grains of powdered Ergot, in a little water and sugar. The remedy operated in a quarter of an hour, and she was delivered in half an hour after. The child was living, but the mother, an hour after its birth, was attacked with dimness of sight and headach, and was not relieved of them till she had vomited the Ergot.

Case XIX.

This woman, having arrived at full time, with her first child, had a slow and tedious labour, with the exception of the last eight hours, when she had pretty strong pains. The head, which had presented its vertex at the os externum for a considerable time, made no further progress; and the contractions of the womb were so weak, although painful,

that labour did not advance. Persuaded that in this advanced state, one or two protracted pains would be sufficient to expel the child, I gave thirty grains of powdered Ergot in a little warm water. The remedy operated in ten minutes; but delivery did not take place till the expiration of forty minutes more. The child died a few minutes before birth; and the mother vomited the Ergot one hour after delivery. This case, which appeared to be well adapted for the employment of the Ergot, does not, however, speak in its favour.

If this death should be attributed to the dose of Ergot, I would ask, why the same dose was not hurtful in Case II.? Why forty grains merely suspended animation in Case V.? Why twenty grains produced the same effect in Case IV.? Why in Case XX. twenty-five grains killed, and the same dose was useful in Case XVIII.? And why, in fine, thirty grains produced no effect in the XXIIId.? These untoward and unequal effects in different persons, are sufficient, one would think, to make us reject the remedy.

Case XX.

The subject of the present case, in labour for the third time, experienced pains at intervals, during two days. The pains having become stronger and more frequent, but by no means effectual, although every thing appeared favourable to delivery, I administered twenty-five grains of powdered Ergot in a little wine. It began to act in a quarter of an hour, and the child was born one hour and a quarter afterwards; and although it had been very vigorous before the administration of the Ergot, it was struck with a mortal asphyxia. The mother vomited two hours after her delivery, without being relieved of a violent headach, which lasted the whole night. The same dose of the spurred rye produced no bad effect in the XVIIIth case.

Case XXI.

This lady, in childbed for the first time, had been dis-

charging her waters from the commencement of her labour. At the expiration of six hours, the orifice of the womb was soft and much dilated, although the child's head had not advanced far into the lower strait. A pretty considerable hemorrhage which accompanied each contraction, and a habit of being bled, contracted by the lady, determined me to take from her fifteen ounces of blood. This bleeding had, at the end of three hours, produced no other effect than to relieve a headach which had existed until then. Thirty grains of Ergot given in a little sweetened water produced no effect until one hour had elapsed; but then the contraction became strong, painful, and lasted half an hour, (not, however, without intervals) which sufficed to produce the delivery of a dead child. The hemorrhage ceased, but the woman was continually troubled with a desire to vomit, and after the birth of the child, she experienced such strong after-pains, that she was reduced to the very brink of the grave. Sinapisms, alcoholic frictions, and large doses of laudanum, were required to restore her.

Case XXII.

Every thing was favourable to delivery, which, however, at the end of two hours, had made but little progress. A half a drachm of pulverized Ergot, given in a little wine, produced delivery (which was of the first child) in less than half an hour, by one long-protracted contraction. The child must have been pretty well compressed; it was born, nevertheless, strong and vigorous, and the mother experienced no bad effects from the medicine.

Case XXIII.

This lady, in labour for the fourth time, suffered violent pain in the back, and the contractions of the womb were so weak, that labour, for a considerable time, had made no progress. She was bled copiously, with a view to relieve the pain of her back, but without much advantage: the inactivity of the womb remained the same. The child's head

having advanced to the lower passage, it was to be presumed that the slightest contraction, if somewhat protracted, would produce its expulsion. In consequence of this, I administered thirty grains of powdered Ergot in two doses. It produced no effect either on the mother or the child, and delivery took place in the ordinary way.

Case XXIV.

The lady, in the present case, having arrived at full time with her first child, had for many hours experienced pains, the more disagreeable as they did not contribute to the advancement of the labour. The head having reached the lower strait, and presenting well, I determined on employing the Ergot, and I gave it after the manner of Dr. Prescott, without any effect.

Case XXV.

Mrs. ——— had been twenty hours in labour with her first child, and the contractions of the womb were so weak, that I had not the slightest expectation of seeing delivery accomplished for many hours. The child's head was at last considerably advanced in the lower passage, and presented well, but the dilatation of the orifice of the uterus did not exceed the size of half a crown, and the woman was, moreover, so weak, that her pulse was scarcely sensible. An infusion of one drachm of powdered Ergot in four ounces of boiling water, given in ounce doses every ten minutes, roused the uterus to action in half an hour, and this action continuing two hours and a half longer, delivery took place, happily to both mother and child.

Case XXVI.

The Ergot, administered in three cases of retention of the placenta, furnished evident proofs of its active qualities. In the last case particularly, the womb had been six hours in a completely inactive state, when the Ergot, given in doses of twelve grains every fifteen minutes, produced, in

half an hour, such strong contractions, that the woman declared, that she suffered more than she had done during labour. The contractions lasted unceasingly nearly two hours, and at last expelled the placenta without affecting the stomach.

Case XXVII.

A lady, in the fourth month of pregnancy, had experienced for the last six weeks an hemorrhage that nothing could arrest: venesection, purgatives, bark, elixir of vitriol, tonics, astringents, frictions, fomentations, all had been employed without success. Believing that this hemorrhage would end fatally, if it were not soon arrested, I ventured on twelve grains of powdered Ergot in a little syrup. It acted on the womb in a quarter of an hour, produced sickness at stomach, and soon after copious vomitings, which put a stop to the contractions of the womb. The hemorrhage, which had also ceased, having returned the next day, the woman took, of her own accord, a similar dose of the Ergot, which not being thrown off by vomiting, brought on abortion in eight hours.

Case XXVIII.

Having renewed the employment of Ergot without success upon sows, and upon a cow, to which I gave six ounces in fourteen hours, I determined to see what would be its effects upon cats. I tried it upon six, all more or less advanced in pregnancy. On the first it acted as an emetic; the second was violently purged; the third, fourth, and fifth, were not at all affected by it, although the last took a double dose of it at once, i. e. two drachms in powder. The sixth, half advanced in pregnancy, to which I gave but one drachm, had her legs paralysed for a short time, in less than half an hour; and abortion took place in twenty-four hours, preceded by considerable hemorrhage.

In taking leave of the Ergot, as well as of the critics, if

there be more of them, I confess that I shall not have fulfilled my intention, if I have not proved in a satisfactory manner that this substance possesses qualities truly active, and often injurious to the fœtus. Deceived by my first experiments upon animals, I believed that the Ergot did not act directly upon the womb; and that, when it did act, it was always by sympathy, and then only when the uterus was predisposed to it, by the irritation natural to it at the end of gestation. The twenty-seventh and twenty-eighth of my present cases, undeceived me; and, corroborated by facts already cited, appear to me to leave nothing more to be desired on the subject. I therefore say, with a sincere belief; that the Ergot is possessed of active properties; that it acts upon the nervous system directly, or by sympathy, according to circumstances; that its action may be salutary or hurtful according to individual susceptibility; that it is not always necessary for the womb to be previously disposed to irritation, in order to be influenced by it; that it acts principally as a tonic; and that its effects, in general, are very prompt, but may sometimes show themselves only at the end of one hour. Its dose cannot be determined with precision. It is better to administer it in infusion than in substance, as already remarked by Dr. Stearns. It appears to act in very small doses, repeated at longer or shorter intervals; as in the dose of ten grains in powder, or a table spoonful every ten minutes, of an infusion made by pouring two ounces of boiling water on thirty grains of the powder. The dose of one or two drachms, infused in two, three, or four ounces of boiling water, as I have prescribed it several times, seems to answer very well, and did not act with more force or promptitude. Whatever be its dose, its effects are always uncertain: it is sometimes inert, but oftener active. The Ergot may act on the mother only, producing giddiness, uneasiness at stomach, headach, and more or less copious vomiting. It may act upon the child, and produce suspended animation

or even death—or it may act upon the mother and child at the same time, and produce all these accidents. Finally, it may act on the womb alone, by ridding it of its contents, whatever they be, without affecting, in any manner, the being that it has protected and nourished for nine months.

These conclusions being the result of the present cases, and of those which have appeared in Vol. XX. of the Medical Repository, I think myself authorized to renounce the employment of the Ergot as a mean to facilitate delivery, because its effects may be injurious to the fœtus, whatever be its *modus operandi* on the mother in general, or on the organ of generation in particular.

On the Mania of Lying-in Women and Nurses, by M. Esquirol, *Physician in Ordinary to the Salpêtrière. Translated for the Medical Repository, from the Annulaire Médico-Chirurgical of the Civil Hospitals of Paris for* 1819.

Although much has been written on the diseases of the puerperal state, little has been said on the mania of lying-in women and nurses. I have therefore chosen this subject, in the hope of advancing our knowledge of a disease which interests us as much, on account of the objects doomed to suffer, as from the frequency of its recurrence.

I shall not speak of that transient delirium which frequently occurs after labour, and is often only a symptom of milk-fever, and usually soon disappears, either on the return of the lochial discharge, where it had been suppressed, or its diminution, where it had been excessive; or the removal of the fever, and the recovery of the patient's strength. Moriceau and other accoucheurs have known this form of mental derangement to appear on the second day of confinement, and disappear on the fourth.

Neither shall I speak of that form of delirium, which has so frequently led mothers in their frenzy to destroy the child they have brought forth. Shame, perplexity of mind, fear, remorse, and crime, are not the sole causes of infanticide; delirium, by agitating and distracting the intellectual functions, often consummates the horrid deed.—An unmarried woman becomes unexpectedly pregnant; she does not conceal the circumstance from her family; she prepares apparel for the child; the evening before her confinement she conducts herself as usual; in the morning she is found in bed, and the child in the privy, disfigured with wounds which appear to have been made with a scissors. Soon after, the girl is seized, and conveyed several miles from her home. On the way, she refuses to be concealed from her enraged neighbours, and continually repeats to those about her, "that no person will harm her," "that she has done nothing." Some days after, she is questioned, and acknowledges the crime: she neither justifies it, nor exhibits the least regret for what she had done; but refuses to take nourishment. I ask, had not this girl a paroxysm of delirium? But these facts appertain to legal medicine, and I leave them.

The number of women attacked with insanity, in consequence of child-bearing, during and after lactation, is much greater than has been commonly supposed. Nearly one-twelfth of the maniacal females received in the hospital of Salpêtrière, owed their derangement to one of these causes. For some years the proportion has been as great as one-tenth. Thus—Of eleven hundred and nineteen admitted during the years 1811, 12, 13, 14, ninety-two were of this kind, and sixty of them were admitted in 1812 and 1813, during which the aggregate number received was six hundred. If a third be subtracted from the total number received in these four years, for those that had passed the fiftieth year, when they are beyond the influence of such causes, the remainder will leave a larger proportion than I have stated. The result of my private practice makes the proportion still greater;

nearly one-seventh. Astruc observes, that depositions and milky congestions are very frequent among the higher classes of society; but it is nevertheless certain, that mental derangement after lactation is not so common among them as the poor; arising, no doubt, from the precautions which they are enabled to employ; whilst nothing of this kind will avail to secure their extreme susceptibility against the dangers of confinement, and of suckling their children.

It is important to ascertain the period of confinement and lactation when this malady is most apt to occur, since it will furnish useful practical indications. Hippocrates, in his third book of Epidemics, relates many cases of serious disease with delirium, in lying-in women during the epidemics which he describes: they were, however, mostly cases of fever. Perhaps case xiv. of the wife of Epicrates was an acute mania: she had been delivered of twins, was attacked with delirium on the second day, and died on the twenty-first. Levret informs us, that fatuity is to be apprehended after delivery, if the lochia be deranged, especially if the breasts remain empty and flaccid. Zimmerman, in his work On Experience, relates examples of insanity and melancholy, preceded by suppression of the lochia: among others he gives the case of a woman who was cured after six months. Dr. Berger published at Gottingen, in 1745, a thesis De Puerperarum Maniâ, which I have not been able to procure. Doublet, in a memoir read before the Royal Society of Medicine in 1786, says, that the *irritatio lactea* is sometimes transferred to the brain, either immediately after delivery, or on the cessation of lactation. In some of these cases, adds this author, there is a fixed pain of the head; in others stupor, with haggard countenance, and wandering of intellect. Of my ninety-two cases, sixteen commenced from the first to the fourth day after delivery.

Puzos says, (Mémoire sur les Dépôts laiteux) that milky depositions do sometimes occur in the brain, and

produce mania by compressing this viscus and distending its fibres. These depositions, according to him, occur from the tenth to the twelfth day after delivery. He relates two cases of this kind. Of my ninety-two, twenty-one occurred from the fifth to the fifteenth day; seventeen from the fifteenth to the sixtieth day, the extreme limit of the lochial flux; nineteen during lactation, and from the second to the twelfth month after delivery; and nineteen immediately after weaning the child.

From these data I conclude, 1°. That women are more liable to mania soon after confinement, than during lactation. 2°. That the danger is lessened in proportion as they are removed from the period of delivery. 3°. That there is much more danger of the disease on weaning the child, than during the period of lactation.

The disease is sometimes announced during the pregnancy by sinister forebodings, as sadness, and groundless and exaggerated alarms; at other times it appears suddenly, without any such warning. At first, there appears to be a febrile condition of the system; the skin is moist and pleasant, the countenance pale, the tongue coated with a white fur, the breasts flaccid, the abdomen neither tense nor painful; sometimes, however, there is great pain in the region of the uterus, the pulse is small, feeble, and corded. In some instances, the derangement is partial, confined to one set of objects; most generally its form is mania, rarely fatuity. This affection has sometimes been confounded with phrenitis; but the redness of the eyes, the dryness of the skin, the tinnitus aurium, the state of the pulse, the subsultus tendinum, the ataxic symptoms, and their rapid increase, serve sufficiently to distinguish this latter disease. Moreover, phrenitis proves fatal in three or four days, rarely continues to the seventh, whereas the duration of mania is often several months, and even longer.

This form of mania differs very little, in its character and progress, from that arising from other causes; yet there is something peculiar in the expression of the coun-

tenance, which will be recognized by those who are familiar with these diseases.

Of the ninety-two cases, which are the subject of this memoir, eight were cases of fatuity, thirty-five of melancholy or partial mania, and forty-nine of mania. It may be asked, if idiotism is never the consequence of child-bearing? I answer, certainly not; for that form of the malady is either connate, or confined to childhood.

Of my 92 cases, 22 were aged from 20 to 25 years.

41	„	„	„	25	„	30
16	„	„	„	30	„	35
11	„	„	„	35	„	40
2	„	„	„	43		

From twenty-five to thirty is the age, then, at which lying-in women, as well as nurses, are most liable to this affection. This is moreover the period of life at which mania from other causes is most likely to happen.

The predisposing causes are, hereditary predisposition, extreme susceptibility, a former attack, especially when placed under similar circumstances, a difficult labour, its return simply, or the function of lactation. These causes have in some instances sufficed to produce the disease. Some women, without any assignable exciting cause, have been known to suffer an attack only on bringing forth a male child, others after every second or third delivery, and some on every return of the fifth month of lactation.

The exciting causes are deviations of regimen and moral affections. Of the former, the effects of cold are most to be feared; whether from exposure to the cool air, the application of cold water, or cutting the hair, &c. Heating medicaments, by suppressing the lochia, expose to the disease. Weaning the child suddenly, without using those precautions which experience has shown to be necessary, is also a frequent cause. Hence, of my ninety-two cases, nineteen occurred immediately, or a few days after that event. We have already remarked, that this cause is much less frequent among the rich than the poor. Of my

ninety-two cases, fourteen were induced by physical causes; and of these fourteen, ten by the effects of cold.

Moral affections are much the most frequent cause of this disease. My experience makes the proportion four to one. Their general and baleful influence has always been appreciated. In Rome it was the custom to declare the habitation of a lying-in woman sacred from intrusion, by suspending a crown over the door; and there still exists a law in Haerlem to guard such habitations against the official visits of bailiffs, and other agents of the police, by displaying in front of the house a certain signal. Van Swieten accounts a deep and sullen anguish to be often the cause of incurable mania in lying-in women; and also, though not so frequently, during lactation, and on the cessation of that function. Of my ninety-two cases, forty-six were caused by moral affections. The fear of a second attack, despair caused by the death of the child, the desertion of the father, rage, fright, sorrow, and domestic broils, are so many exciting causes. Of thirteen cases arising from moral causes admitted in the Salpêtrière in 1814, eleven were induced by fright. The frequency of this cause was also shown in 1815. In proportion as the state of confinement and lactation increase the susceptibility and mobility of women, by rendering them more accessible to the influence of surrounding accidental circumstances, are these causes efficient in producing the disease. They, especially the moral causes, are most powerful when suddenly applied.

A woman is happily confined, and on the day after perfumes her bed with some odoriferous liquor, on which the lochia and secretion of milk become suppressed, mania supervenes, which continues for ten months, and is only removed on the subsidence of a mucous fever.—A husband throws a bucket of cold water over his wife, who had been confined the night before, and she is soon after seized with incurable mania.—A girl, aged 18 years, escaping the vigilance of her parents, is delivered of her child during ex-

treme cold weather in a barn, and immediately after passes to her chamber: she becomes maniacal, and only recovers the use of her reason a year after.—A nurse is overtaken in a storm; in hastening home, she becomes heated, and wades through a brook: in consequence of which, the secretion of milk is suppressed, and she is seized with melancholy.—Another is frightened by a clap of thunder, has her milk suppressed, and loses her reason.

When mania appears in the puerperal state, the lochia are usually diminished, or altogether suppressed; the secretion of milk does not take place, and the breasts become flaccid. In some instances the lochia are only disturbed; in other rare cases they are even more abundant than natural, and present a hemorrhagic character. When the disease attacks during the period of lactation, the milk, in some instances, is suppressed, in others, only diminished in quantity, and deprived of its healthy and benign qualities, which causes the infant to refuse the breast. Very frequently, however, there is no change in either quantity or quality, and the child sucks as usual.

It may be doubted, whether the derangement of the function of lactation be the cause or consequence of the mental alienation. In the greatest number of cases the milk is diminished or suppressed, and altered in its qualities before the appearance of the disease; but there are many cases also in which these changes take place only after the maniacal accession. It is very certain that the disease occurs most frequently in those females who do not suckle their infants: hence its frequency in the unmarried. Of my ninety-two cases, twenty-nine were unmarried; a large proportion, when it is considered that child-bearing women are generally married.

It has also been a question, how far the milk acts as a foreign body, by being translated to other organs. The ancients believed that this was frequently the case, and attributed to it all the irregularities of its secretion. Sennertus regarded this form of mania as caused by the va-

pours of the uterus affecting the head. Levret assures us, that he has found milk in the cavity of the cranium. Boerhaave objects to considering the accidents incident to the puerperal state as different affections: he believes them to be produced by the same cause, and to be modifications of the same disease. Almost every accoucheur has considered them as arising from a metastasis of the milk, and this opinion has been adopted by the vulgar, and served to console many a sufferer, whose malady was incurable. Modern researches, especially those of Chaussier and Bichat, have shown that this secretion is never found in the abdomen of puerperal women who had died of peritonitis. Neither does it ever exist in the cavity of the cranium. The moderns have thought that there is in the puerperal period a milky diathesis, which modifies all the secretions, and tends to produce diseases of a peculiar character.

Hippocrates lib. v. aph. 40, says, that women who have a discharge of blood from the breasts are threatened with mania. Nevertheless, we sometimes see the catamenia vicarious from these organs, without any such result; and Planchon (Journal Général de Médecine) assures us, that Dr. Pieters of Louvain knew a nurse to render blood instead of milk, without suffering mental derangement. Doublet has frequently observed the same thing at the Hospital of the Vaugirard, in cases of inflamed breasts, and without being followed by ill consequences.

Puerperal mania is more frequently cured, than that arising from other causes; for generally more than one-half recover. Of my ninety-two cases, fifty-five recovered, which is one-seventh of all the recoveries at the Salpêtrière during the four years. The cure is usually attended by a return of the lochia, the re-establishment of the secretion of milk where it has been suppressed, a copious leucorrhæal flow, alvine dejections, in some instances mucous, in others bloody, the reappearance of the catamenia, and more rarely by a return of pregnancy.

The duration of the disease is very variable. Of the fifty-five recoveries,

4 had place in the 1st month,
7 do. 2d do.
6 do. 3d do.
7 do. 4th do.
5 do. 5th do.
9 do. 6th do.
17 do. in the succeeding months,
2 do. after two years.

From which it appears that thirty-eight, or two-thirds of the whole number, occurred in the first six months.

If recoveries are frequent, it must also be acknowledged that the disease is very liable to return, especially when the predisposition is great; and this is only to be guarded against by avoiding, on the return of pregnancy, those circumstances which had provoked the first attack.

On collating my notes, I was surprised to find, that of my ninety-two cases, only six had proved fatal.

1 six months after confinement,
1 do. a year,
2 do. eighteen months,
1 do. three years,
1 do. five years.

The mortality, then, was one-eighteenth of these cases, and one-fifty-third of the whole number of maniacs in the hospital during the four years. How is it that affections of the abdominal viscera occurring in child-bed women, are so frequently fatal, and those of the cerebral organ, under the same circumstances, very seldom so? The solution of this question is perhaps to be sought for in the physiology of the brain, and the study of the influence of that organ on organic life.

The autopsic examination of these cases has offered nothing of interest, or which could lead to a knowledge of the cause or seat of the disease. Selle observes, in his Collection of Cases, that he found a large quantity of pus

in the brain of a woman who had died maniacal from a second attack after confinement. I have found in these cases of insanity, as well as in those arising from other causes, an effusion of albumen between the meninges; but never any substance which resembled milk. No doubt similar depositions have deceived authors. Besides, it would be as extraordinary to find milk in the cavity of the brain in these instances, as to find menstrual blood there, in mania caused by a suppression of the catamenia.

The treatment of puerperal mania has been various, and authors, without reflecting on the different causes which produce the disease, have too generally deemed it incurable. Even Puzos, after being frequently baffled in its treatment, appears to have been of this opinion, and thinks it is only to be overcome by the free and repeated employment of bleeding and purgatives. Most authors have employed the same means, and to excess, deceived, no doubt, by the violence of the symptoms and the suppression of the lochia; whereas this last symptom is by no means a constant attendant; but, on the contrary, there is sometimes a too abundant flow of this discharge. Haller relates a case of this kind; and in one of mine, the attack had been preceded for eight days by a uterine hemorrhagy. In those cases where the secretion of milk was suppressed, it was usual to endeavour to recall it by the use of purgatives, the application of cupping-glasses to the breasts, and the employment of suction. Boerhaave and Van Swieten, (Aph. 1332,) advise the cautious use of the lancet, even where the lochia are suppressed; for, say they, the risk of debilitating is greater than the advantage to be derived from the remedy. Indeed, the debility and exhaustion consequent to the puerperal state, ought to guard us against the too liberal employment of depleting remedies, and bleeding especially should be employed with the greatest caution. Even in cases of plethora, and a sanguine temperament, it would be most adviseable to have recourse to the application of leeches to the labia or thighs, and, in

general, cupping, blisters and sinapisms sometimes to the legs or thighs, in others to the back of the neck, (joined with sudorific or mild purgative ptisans, according to the state of the patient,) should be preferred to more active means. Of late, drastic purgatives have given place to laxative medicines. Some cases have been cured, merely by persisting in the daily use of purgative enemata and an antiphlogistic diet. In others, where the lymphatic temperament predominated, the repetition of emetics has been successful. Blisters are of most service in the advanced stage, when the affection has become chronic; and the same observation will apply to the use of the tepid and hot bath. A new pregnancy and nursing the child have, in some instances, removed the disease, and hence been recommended as a general means of cure; but I do not think upon good grounds; for in those cases, the cause must have been, as it were, accidental, and could not have depended on any anterior predisposing vice of habit. After the disease has continued for some time, especially in nursing women, it would be well to attempt the re-establishment of the menstrual flux, by the application of leeches to the labia, and the use of emmenagogue medicines. Attention to regimen and moral treatment are not to be neglected, although it must be confessed this last has not been so frequently attended with success as in mania from other causes.

REVIEW.

Traité de la Fièvre Jaune, par Jean Devèze, Docteur en Médecine de la Faculté de Paris, Médecin du Château des Tuileries, &c. &c. 8vo. pp. 304. Paris, chez Aimé Comte.

"Je discute une question qu'on a toujours cherché à embrouiller, et mon but est de l'eclaircir." We regret to acknowledge that a large proportion of the treatises which have appeared on the yellow fever, justifies this reproach of our author, but at the same time it gives us pleasure to admit, that his own affords internal evidence of the sincerity of that part of the remark which applies to himself. It is a lamentable fact, for which we can assign no sufficient reason, that medical doctrines are usually discussed with more irritation and acrimony than those of any other branch of natural science. It would indeed seem, as if the advocates of a medical theory identified their feelings with their subject, and staked their character on its success; while they viewed their opponents as not so much searching after truth, as engaged in a conflict for individual superiority, and the glory of giving law to the whole medical community. Passing by the various discussions on minor topics, in confirmation of our opinion, what abundant testimony do the passionate disputes, and furious contentions, which protracted the doctrinal warfare of the humorists and solidists, furnish on this point? which, while it must be confessed they benefitted the cause of general science, disturbed the naturally tranquil vocation of our divine art, and represented the benefactors of mankind in the odious attitude of angry combatants. And for what? that we might learn a lesson of humility, and forbear the attempt to arrest the march of opinion in matters appertaining to a science progres-

sively improving; for its light, instead of illuming, has consumed, and consigned to oblivion all the causes of their most violent controversies. The discussions on the attribute of the vaccine virus, were as rude and intemperate as any which had preceded them; but the positive and undisputed facts which time assisted to develope, were brought to bear directly on the question, while the circumscribed nature of the subject admitting of little that was conjectural, soon brought the parties to an issue, from which there was no appeal, and quickly silenced the cavillings of the interested, by establishing on an imperishable basis the utility of the discovery.

But such has not been the event in relation to the subject we are about to examine—the investigation of the origin and nature of the yellow fever does not possess these evident advantages: it necessarily leads into the wide field of doctrine concerning fevers in general, and all those diseases which are in any way connected with them—embraces such a mass of fact, often showing seeming discrepancies, and admits of so much hypothesis, and so many means of evading unwelcome results, by taking shelter under confused and ambiguous language, that no subject has more exercised the learning and ingenuity of modern disputants, or been more hotly contested, or more ably and obstinately defended. A part of all this may be attributed to the predilection of some men to defend doctrines which have been sanctioned by antiquity, although these may have arisen at a time when, comparatively speaking, the intellect was merged in ignorance, or trammelled by superstition—and a part also to the advantage which some have thought to derive, by nursing the prejudices of the public, in order, that by operating upon their fears, which are the necessary offspring of their unacquaintance with the subject, they may appear to be, not only the exclusive advocates of popular doctrines, but the only candidates for public favour. On this account it is, that in exhibiting a view of the present advanced state of

the yellow fever discussion, the few remaining advocates in this country of the barefaced illusions of *human contagion, as applied to this fever*, claim less indulgence than they would, under other circumstances, and with a more disinterested creed, have a right to expect.

The work before us presents peculiar claims to the candid examination of American physicians, not only as investigating a disease with which many of them are practically acquainted, and exhibiting and enforcing doctrines, which for many years they have defended, in opposition to the received theories and opinions of European writers, founded, for the most part, on the relations of others, and the forced analogies with fevers of a very different character; but as coming from a person of high professional credit and responsibility near the French monarch, and professing to give the result of his reflection and research during a period of twenty years in which he had been conversant with it—embracing fifteen years experience of its annual recurrence in the island of St. Domingo, and two of its most desolating visitations in the city of Philadelphia.

During the year 1793, the author had the medical charge of the hospital at Bush-hill, situated a short distance from the city, devoted to the reception of yellow fever patients, and he there possessed the amplest opportunities of observing the disease, and tracing its origin and propagation: they are the occurrences of that endemic more particularly which he records and elucidates in the volume before us. While we allow that this work exhibits a clear, masterly, and more comprehensive view of the origin, character, and extent of this fever, than any book which we have had the good fortune to meet with, we must remark, that we do not find him inculcating any other views, either of doctrine or practice, than those long ago ably maintained by our countrymen, Rush, Miller, Mitchill, Bayley, and others. We have not been able to obtain a copy of the author's first publication on this subject, and therefore cannot pretend to decide the claim of priority for which he contends; but at any rate, the production

of one of these gentlemen must have appeared within a year, or at most two years, after his first publication; and we think that he has been wanting in that literary courtesy which should always distinguish professional men, in not attributing to them that participation in the subversion of ancient error and delusion, which we believe is justly their due, although they may have entered the lists somewhat later. Two of them he has wholly omitted to notice, and the others he merely quotes on minor points of doctrine. We hope this did not proceed from any feeling of resentment towards American physicians, arising from the disrespect which the medical men of Philadelphia manifested towards him during the period of his early residence in that city: if it did; we think that a little cool reflection in a sensible man should have been able to overcome it, especially as he admits, that himself and fellow-sufferers, flying in dismay from one of the most indiscriminate and merciless massacres in modern times, have nothing to complain of, but much to commend, on the score of sympathy and humanity, from the people generally.—But we willingly leave a subject that partakes too much of personal feeling, and proceed to lay before our readers, as succinctly as possible, a view of the leading facts and doctrines in the order of the work.

He first enumerates the different appellations by which the disease has been designated, as Coup de Barre, by the early French voyagers; Plague; Black Vomiting; Mal de Siam; Mal de Matelot; Typhus Icterodes, by *Sauvages;* Continua Putrida Icterodes Caroliniensis, by *M'Bride;* Elodes Icterodes, by *Vogel;* Febris Maligna Flava Indiæ Occidentalis, *Mac Kittrick;* Febris Maligna Biliosa Americæ, *Moultrie;* Synochus Icterodes Endemicus, *Moseley;* Remitting Yellow Fever, *Rush;* Malignant Pestilential Fever, *Chisholm;* Fievre Gastro-adynamique, *Pinel;* Typhus d' Amerique, *Bally;* and then adopts that of Yellow Fever, for no better reason than that it has been the term most generally employed. To us it appears very objectionable

to designate a disease by a symptom which is neither essential nor constant, and cannot with any propriety be admitted into a nosological nomenclature. We can see no objections to employing the denomination given to it by Sauvages, *Typhus Icterodes*, or that employed by Pinel, *Fièvre Gastro-adynamique*, either of which are as appropriate as any of those now used by common consent, to designate other fevers: For the concurrent testimony of all authors who have been conversant with the disease, goes incontestably to prove that it should be classed in the order of ataxic fevers of Pinel, and make a species or variety of the Typhus gravior in Cullen's arrangement.

In the chapter devoted to the topography of Philadelphia, he shows that there existed in that city abundant causes for the generation of malignant fever, especially in those parts where the disease has always originated, and chiefly prevailed—as the narrow, low, ill ventilated, crowded, and filthy condition of the streets—the accumulation in the docks adjacent of putrescent materials, exposed at low water to the rays of a hot sun—the great and sudden transitions in the temperature of the weather; and the level condition of the scite on which the city is built. On these and other causes of insalubrity, we fully agree with the author; but on some points, chiefly of minor importance, we suspect him of quoting from memory, or giving us conjecture; as when he states the maximum heat of summer at 30° Farh.; accuses the inhabitants of intoxication, want of cleanliness, the habitual use of unripe fruits, and bread of an inferior quality, &c. now it is well known, that the heat of many days during the summer exceeds that point, and not unusually, in the hottest weather, the thermometer ranges beyond 90°. Such observations, and some others we could mention, are as little applicable to Philadelphia, as to any other city of this country or Europe. A more moral and better regulated community can be found nowhere; and the superior excellency of their fruits and bread stuffs we have always considered to be their peculiar boast. But

these are trifling inaccuracies, which can have little influence on the main design of the work; we notice them merely to call the writer's attention to this section, which appears to us the most defective in the book, and to recommend him, before he issues another edition, to take measures to obtain from some resident, a fuller and more correct account of that city, which, we are confident, would further illustrate his doctrinal remarks, and add to their value.

The description of the disease, as it appeared in Philadelphia in 1793, being one of the most circumstantial and lucid details of the symptoms, progress, and character of this fever that has yet been given to the public, we are induced to insert it at length, and we earnestly recommend it to the study of the American physician.

"The winter preceding the endemic, had been mild, the spring early, and the summer excessively hot, attended with a long drought, and a low state of the streams. A little before the appearance of the disease, an epizooty, especially among cats, prevailed; inflammatory cynanche was very rife; the air swarmed with an extraordinary quantity of insects, and great numbers of dead fish were observed floating on the surface of the rivers. The farmers also complained of the oppressive heat, causing a scantier perspiration than usual.

"The fever exhibited in its progress three distinct periods or stages. The premonitory symptoms of the first stage were generally slight; in some cases they were those of languor, lassitude, and anorexia, which could often be attributed to preceding debility or terror; but most generally the patients were seized suddenly, without any such warning, with great distress of the head, and such excruciating pains in the loins and extremities, as to cause them to complain that they felt as if these parts had been beaten or bruised. A heat excessive, dry, and acrid, alternating with rapid and irregular rigours, imparted to the hand of the physician a peculiar sensation; the face was red and turgid with blood,

and the eyes fixed, watery and wild. Sometimes the heat of the internal parts of the thorax and abdomen was so great, as to cause indescribable anguish, whilst that of the extremities was moderate, or even below the natural standard; the thirst was tormenting; the deglutition difficult, the respiration straitened, laborious, and interrupted, and on applying the back of the hand near the patient's mouth, the air expired felt hot; it was this extreme heat which parched and irritated the throat, tongue, lips, and nasal cavities: the patient appeared greatly oppressed, and sighed much, as if to overcome the obstructed and laborious respiration which threatened suffocation: the tongue, which at first was red and dry, soon became covered, as well as the teeth and lips, with a yellow slimy material, which soon changed to a black. The pain, tension, and renitency of the epigastric region announced the violent vomiting of which they were the precursors; the urine was small in quantity, high coloured, and scalding; watchfulness and disturbed sleep, pulse strong, hard, and quick, constituted, with the foregoing symptoms, the first stage of this disease, which continued one, two, or three days, rarely more; I have, however, known it to extend to the fifth.

"The second stage commenced with a subsidence of all the symptoms of irritation; the face now became less red, indicating the presence of an alarming malady; the respiration continued difficult; the heat acrid and mordicant, the pulse quick and small, and the tongue, lips, and teeth more and more covered with a black, thick, and slimy matter: the vomiting continued, and the matter ejected was variable; sometimes whitish and very acid, setting the teeth on edge; sometimes of pure bile, and at others, black and glareous, resembling ink grounds, emitting an hepatic odour, and so acrid as to excoriate the throat, tongue, and lips. The thirst remained intense, but the patient refused to take drinks on account of the great distress their immediate rejection occasioned. Alvine discharges sometimes had place in the first stage, but more usually not un-

til the second, preceded and attended with pains in the lumbar region. They varied much in their nature and colour, at first liquid, frequent, and slimy; later, yellow and black; and in the third stage bloody, and sometimes blood only. The urine was high-coloured and turbid, generally without sediment, sometimes covered with a thin pellicle, and in some few instances depositing a sediment, differing in each case. The sleep continued much disturbed; and when, as by chance, the patient obtained a short slumber, it was interrupted by distressing dreams, which harassed him more than constant watchfulness. Yellowness of the surface sometimes came on during the first stage, but more frequently not until this; it was preceded by restlessness and lassitude, first appearing in the eyes, and afterward extending over the whole body. The punctures made by venesection were now surrounded by a livid circle, and re-opened, so that the flow of blood was arrested with difficulty. This accident was especially apt to happen when the patient had been bled late in the disease. The duration of this stage varied very much, and was marked more by the concourse of symptoms than by the length of time it had continued.

"The third stage may be considered as a continued and aggravated form of the second, accompanied by a greater number of ataxic and malignant symptoms. The face now became hippocratic, the pulse weak, small, and intermitting, the respiration slow and stertorous, the vomiting frequent and alarming, the alvine dejections fetid, cadaverous, and involuntary, and the urine black or bloody, involuntary, or oftener suppressed. Passive hemorrhages from the nose, and a continued oozing of black blood from the tongue, lips, and gums, subsultus tendinum, and finally loss of sense and motion, constituted this stage of the disease. If these symptoms were not quickly arrested, the body became covered with livid blotches, which soon gangrened; a most fetid odour exhaled from the whole person before death, and if the hand were applied near the patient's mouth, the breath was found to be cold. The last symptoms were

particularly observed in those whose disease had been preceded by great lassitude and feebleness. In some cases, fortunately rare, the disease deviated from its ordinary course. The patient who had previously enjoyed good health, was suddenly seized with unspeakable terror and violent tremor, quickly lost all power of sense and motion, and died in a few hours. In others the patient complained of some slight indisposition, which he attributed to some trifling cause, and believed would disappear without medicine. If these persons were examined closely, they were found in an alarming condition, sometimes with the pulse slow, feeble, and scarcely perceptible; at other times intermitting, small, and frequent; the skin in some, dry and hot, in others cold, and covered with a viscid perspiration; tongue sometimes natural, sometimes dry and red, and in others pale and tremulous; lips pale or purple, countenance much altered, expressing in some sadness, in others a forced gayety, with slight tremor, and subsultus tendinum, showing the powerful and dangerous impression which the deleterious cause had made on the whole system.

"It was impossible to assign the type of the disease; in some cases it was continued, oftener remittent, and in a few intermittent. The prognosis was generally very unfavourable, on account of the violence and rapidity with which the symptoms supervened. If the fever was protracted beyond the 7th, 9th, or 11th day, and the patient was naturally of a good habit, there was much to hope, especially if the surface of the body became moist; whereas, lassitude and feebleness, with a rapid progress of the symptoms of the second stage, foreboded a fatal issue. The termination by crisis was usually very imperfect, especially by the skin and kidneys. The vomitings were symptomatic, and seldom critical; but the hemorrhages, although symptomatic, were more frequently favourable, when not evidently passive, and distilling by drops from the nose and mouth—the appearance of the catamenia were often also very beneficial. The appearance of jaundice marked the violence

of the disease, and was never critical: a suppression of the alvine discharges was almost always followed by coma and death, and when excessive, or of a bad quality, they were usually attended with gangrenous appearances of blistered, and other parts, which indicated a fatal result. A translation of the pain to the head was also very unfavourable. No case of bubo or carbuncle occurred in my practice, and but one of swelled parotid, which suppurated and terminated the disease favourably. Yellowness of the skin, and black vomiting were the symptoms which were most constantly present, but they were not essential to the disease, for frequently one of them was wanting, and more rarely, both."

Such were the symptoms, progress, and termination of the disease, as observed in Philadelphia in 1793. The same general characteristics, its remittent type, its rapid and violent march, its first stage of erethrism quickly merged in symptoms of the most malignant and alarming character; sometimes confounding in one short period, it might almost be said in the same moment, its invasion, its different stages, and the death of the patient—in others running its course like an ordinary remittent fever, gently and regularly to a safe termination, have been recorded by all writers who have observed the disease fully developed, and raging epidemically, from the diffusion and intensity of its causes.

The results of the author's autopsic examinations are very satisfactory, and in general accord with those given by preceding writers. He found the brain in most instances in a healthy state, or with only slight morbid alterations; the lungs had generally suffered great derangement, were diminished in volume, gangrenous, engorged with black blood, and their texture easily broken down; in some few instances, however, they were in a healthy condition. The heart most commonly, with the right auricle distended with black blood, and the other cavities empty and flaccid. The stomach and intestines, in almost every case, highly

inflamed, much thickened, gangrenous in many parts, and containing fetid, black, and bloody matter, with coagula. The liver often enlarged and dark coloured, though sometimes in a healthy state. The spleen and pancreas rarely altered, but in some few instances enlarged, and the former engorged with blood, and rotten in its texture. Kidneys rarely diseased. The bladder generally inflamed, contracted, and containing black, bloody, and fetid urine. He confirms the observations of Savarésy, Bally, and others, that some persons, dead of this disease, exhibited, on inspection, no organic lesion whatever, and this especially occurred where the fever had attacked with extraordinary violence, and proved quickly fatal.

Our author assigns plethora, and great atmospheric heat, as the great predisposing causes of this fever. Great heat operating on habits unaccustomed to its effects, and to the influence of a vitiated atmosphere, which usually exists where this disease is prevalent. His own extensive experience in the West Indies has afforded him abundant evidence of the very general exemption of persons long resident in that hot climate, which is particularly unhealthy *only* to new comers from northern regions. He denies, and it appears to us on good grounds, the prevailing opinion of the unsusceptibility of negroes, other than that, which they acquire in common with the whites from being acclimated; and in proof of this doctrine, he states that in Philadelphia, in 1793, the negroes suffered equally with the white population. That robust and plethoric constitutions, especially of the bilious or sanguine temperament, are most liable to the disease, has been long admitted, as well as the comparative exemption of females, as being the reverse of this condition. To this chiefly, is to be ascribed the fact which he gives on the authority of Berthe, that in one of the epidemics of Cadiz, only 1577 females perished out of 7387 victims; and at Seville 3672 out of 14685.

He contends that the exciting cause of this fever is noxious exhalations emitted from dead animal and vegetable mat-

ter, acted on by heat and moisture, and undergoing fermentative changes: which putrifying materials he terms the seat or centre of infection, and the fevers arising from this cause *infectious*, in contradistinction to those that are *contagious*, or propagated from person to person, by means of a specific morbid secretion emanating from a diseased person, and which secretion produces the same malady in the person exposed to its influence. He details in a lucid and satisfactory manner, the peculiar circumstances and distinguishing characteristics of each of these classes of fevers, and shows, that in their origin, and mode of communication, they possess little in common, though they have heretofore been confounded, by using the terms to express these attributes in a loose and indefinite sense, and by failing to trace and appreciate those phenomena which a clear understanding of them would have directed. In the class of infectious fevers he enumerates *intermitting* and *remitting fevers*, *yellow fever*, *plague*, and *typhus*; contending that they all proceed from the same causes, differing only in intensity, and are different grades of the same disease.

This doctrine of the unity of fever was long ago taught by our illustrious countryman Rush, and has something in it so extremely captivating to an enthusiastic and generalizing mind, that it requires all the scepticism and severity of a scrutinizing philosophy to guard against its fascinations. That almost every endemic yellow fever has furnished cases of ordinary intermitting and remitting fevers, occurring at the same time, and at the same place, and apparently from the same cause, cannot be doubted; but that these, and all the different grades of typhus, are produced exclusively, and in every instance, by miasmatic exhalations, is by no means made out; and it is fair to conclude, that as fevers, differing widely in character, originate from deleterious exhalations, that these exhalations differ also essentially in properties, although we may not be able to discover or distinguish them.

On the whole, therefore, we perfectly agree with our

author in assigning a distinctive character to that class of fevers which he has denominated infectious; that they are totally distinct in their origin and mode of propagation from those which are strictly contagious, as small-pox, measles, &c. and we are willing to leave to future investigations the task of proving their identity, content to adopt, for the present, as the best we have met with, their further subdivisions and distinctions, as unfolded by Dr. Miller, in his excellent paper on this subject.*

That gentleman divides the infectious sources of fever into two kinds, viz. *Atmosphæra Koino-Miasmatica*, or air charged with miasmata exhaled by solar heat from the surface of swampy grounds, or from masses of filth overspreading the open area of cities, giving rise to intermittent, remittent, yellow fever, and plague: and *Atmosphæra Idio-Miasmatica*, air charged with miasmata emitted from, and surrounding the body, clothes, bedding, and furniture of persons immersed in the filth of their own excretions, and of those associated in the same family with them, accumulated, long retained, and acted upon by animal heat, which produces all the varieties of typhus.

It is a great point gained towards the right understanding of those fevers, about which there has been so much controversy, if we can affix appropriate and well-defined terms, by which the characteristic and peculiar circumstances of each may be designated; and it is sincerely to be wished, that the distinction between contagious and infectious diseases, now so ably drawn and illustrated by our author, would henceforth be kept in view by succeeding writers. The merit of pointing out those differences, and recommending the use of those terms, with these restrictions, we believe belongs to the late Dr. Richard Bayley of this city, and will be found in his Account of the Yellow Fever of New-York,

* Vide Appendix by Edward Miller, M D. Prof. Prac. Phys. in the University of New-York, in his Edition of Thomas's Practice of Physic, published in New-York, 1811.

in 1795. Dr. Adams has also adopted them in his work on Morbid Poisons, without mentioning Dr. Bayley, or, perhaps, being aware of his previous labours.

Dr. Hosack of this city, in a letter to Dr. Chisholm, in 1808, acknowledges the advantages of this distinction, and admits that much of the dispute concerning the contagious nature of diseases, has arisen from the indefinite manner in which those terms have been used. In order to reconcile facts, and obviate those difficulties, he proposes a new arrangement, in which his first business is to recur to the original, vague, and synonymous use of these words. As this gentleman is almost the only remaining advocate in this country for the contagious nature of yellow fever, his view of the subject may be considered, a just expose of the actual state of this controverted question, as maintained by one, whose frequent opportunities of observing the disease, and collating the facts, have forced upon him concessions, that necessarily lead to establish its infectious character simply—which admissions, European writers, whose practical acquaintance with the disease has for the most part been limited, still persist in denying.

According to this gentleman's notions, communicable diseases are to be divided into three classes, viz. 1st. Diseases communicated by contact exclusively, as psora, syphilis, &c. 2d. Diseases communicated by contact and by the atmosphere, whether pure or impure, as small pox, measles, &c. And, 3d. Diseases communicated through the medium of an impure atmosphere, as plague, yellow fever, typhus, and dysentery.

The distinction here set up for the conditional contagious character of infectious fevers included in this third division, is impugned and condemned by nearly all the advocates for the contagious character of these fevers, by their allowing, that noxious exhalations aided by predisposition, is alone sufficient to produce them *de novo;* and even Dr. Hosack professes himself of this opinion. Thus, Blane in his Elements of Medical Logic, says, that all contagious

fevers originate from vitiated human effluvia, generated by the living body under circumstances of crowding, filth, want of ventilation, &c. Chisholm, on the other hand, objects to classing dysentery with yellow fever, typhus, and plague, because it most frequently originates from marsh effluvia, and he does not believe that a disease produced by *such a cause*, can *ever* be contagious.

Independently of the mass of clear and satisfactory testimony, proving the infectious character of yellow fever, contained in the work of Dr. Devèze, in the volumes of this Journal, and in the pages of almost every author who has had an opportunity of tracing the disease; we do not know a better collection of facts from various writers, to show the important and efficient agency of deleterious exhalations, in originating and propagating the different pestilential diseases which have desolated the cities of ancient and modern Europe, than the paper of Dr. Hosack. The noted cases of the felons brought into a court of justice, at the Oxford Assizes, in 1571, communicating a malignant fever to those near them, has been often and exultingly quoted, to prove the contagion of those fevers; and yet when one views all the circumstances which attended these cases, there cannot be a stronger elucidation of their *simply* infectious character. The felons had been long confined in crowded and ill-ventilated apartments, living in a most filthy condition, with their apparel imbued with their own excretions, and the putrid emanations which surrounded them: being accustomed to this state of things, they received no injury; but on bringing them into court, the exhalations emitted from their filthy clothes, communicated an infectious, and malignant fever to several of the officers and bystanders—the felons themselves being healthy, had no disease to communicate by contagion. Now, if it be admitted, that all the diseases of this third class do frequently originate from noxious exhalations, without the aid of a contagious virus, what necessity is there, for calling in an imaginary power, seeing that of it-

self this power is insufficient, and must always be connected with a cause, which alone, is competent to produce the effect? The position is wholly untenable, and could never have been taken had its consequences been examined;—but this is not the only objection to the theory. The perfect exemption with which yellow fever patients have always been approached, (for the pretended exceptions do not amount to as many as the cases of a second attack of small pox,) when removed but a short distance from the infected spot, has led our theorist to explain the fact, by saying, that pure air destroys the contagious virus. If this explanation be examined, it will be found to be the reverse of what happens under similar circumstances in other diseases, and not conformable to what we know of the properties of the atmosphere: for instance, it has never been denied, that those diseases which are allowed by all to be contagious, are as certainly and quickly communicated in the pure air of the country, as amidst the foul vapours of a city; and from what we know of the atmospheric constitution, how in the nature of things could it be otherwise? or, if there was a difference, we should expect à priori, that that difference would be in favour of an impure air, as the additional material it contains, *might* possess the property of rendering the contagious virus inert. But if, on the contrary, the ordinary constituents of the atmosphere could neutralize such virus, then, it would be impossible to have it transmitted through the air under any circumstances; for pneumatic experiments have shown that the ordinary atmospheric constituents invariably exist in the same proportion in every situation, whether over marshes, in crowded cities, or on mountains; and of course must always be present to produce their neutralizing effect. This difficulty, however, is attempted to be obviated, by supposing a chemical combination to be formed between the virus and the noxious emanations; so that as it is the property of chemical union to produce a compound differing from either of the components, we are led to the ab-

surd result, that the matter generated by these diseases, is innoxious, and incapable of producing them; but acquires its deleterious property on its way to another person, by forming a new substance. Seeing this to be the case, we hope, for consistency's sake, to hear no more of the disease being communicated, in some rare instances, without the aid of impure air, as in all *such instances* a necessary ingredient to make contagious matter must be wanting, and a single exception of this kind would utterly overturn this fine theory. The doctrine of unconditional contagion possesses one merit at least; it is consistent with itself; whereas, the doctrine of our professor carries with it its own refutation, and leads to unphilosophical and absurd conclusions.

We return with pleasure from this long digression to the text of our author. After having pointed out, in lucid and simple detail, the laws which distinguish infectious and contagious fevers, he proceeds to the consideration of the phenomena of yellow fever, as they are presented in the course of an epidemic, which he divides into four periods, viz. the rise of the disease, its propagation, prevalence, and subsidence.

1°. Yellow fever originates, like other infectious fevers, from deleterious exhalations, emitted from dead animal and vegetable matter, acted upon by heat and moisture, and its progress is always suddenly arrested by the cold and frosts of winter; whereas small pox, measles, pertussis, and other contagious diseases, have never been known to arise *de novo* from any combination of natural causes; they, moreover, pursue their course unchecked, and with equal violence, under the heat of the equator and the ice of the poles, as long as there are proper subjects to receive the disease, or to continue its progress. A third attribute which yellow fever possesses in common with other infectious fevers, is the great influence which moral causes exert over its rise and propagation. This influence was strikingly illustrated in the epidemics of Philadelphia, Cadiz, and St.

Domingo; and the great susceptibility of the timid to receive the infection, has always been the common remark of the vulgar, as well as physicians. Nothing of this kind has been observed in small pox contagions, which have always been thought to produce their effects wholly independent of such causes. Wherever yellow fever has appeared, there have not been wanting persons, and those too in some instances *contagionists*, who have pointed out sufficient local causes to account for its generation; yet such has been their eagerness to assign a foreign origin, that where a vessel lately arrived from a hot climate could not be found, upon which to fix the imputation, the advocates of exclusive importation have not scrupled to say, that the virus had been imported in a preceding year, or lain dormant for two or even three years. When the disease first appeared in Philadelphia in 1793, the brig Mary was accused of having introduced it, but our author happening to have been a passenger in her, testified that no malignant fever had prevailed on board, during the voyage: then a French vessel was named, and she too, on examination, being exculpated, the Hankey was resorted to, as the source of contagion. Dr. Smith and Mr. Webster afterwards proved, that there was as little foundation to attribute the fever to this last vessel, as to either of the others. What occurred in Philadelphia on this head, is a fair specimen of what has happened in every place where the disease has prevailed; the same eagerness to search abroad for what may be found at home, and the same removal of accusation from vessel to vessel, until one could be found, whose dubious history could render her a fit subject to bear the imputation.

Among the frivolous attempts to account for the production of infectious diseases by contagious causes, may be classed the following instances given in the work before us. The plague in Paris, 1507, was attributed to some tow, which the workmen had met with, while engaged in taking down an old wall, and which was supposed to have been deposited there by rats seven years before, when the last

pestilential disease prevailed in that city.—Kirker supposes the same disease to have been brought to an Italian town, by a raven, which had fallen dead in a public square, and communicated the disease to the boys who had pulled off the feathers.—A merchant at Leghorn is alleged to have taken the yellow fever, by writing with a quill that had been imported from America.—One of the epidemics of Boston was supposed to have originated from a trunk of clothes which belonged to a person who had arrived in town, sickened, and died there; but had not communicated disease to any of those who had approached or administered to him during his illness.

The following instances are selected, from many others, on account of the few and precise circumstances that attended them, to show the generation of yellow fever, by exhalations from decaying animal and vegetable matter. The ship General Green, a new vessel, left Newport, R. I. for the Havana; on the voyage they experienced much bad weather, the vessel proved leaky, and the provisions became spoiled, and emitted putrid exhalations; the consequence was, that the yellow fever appeared among the crew before they arrived at their port of destination.—The English ship Hibbert arriving in the port of New-York from Portsmouth, England, in ballast, was found, on examination, to contain much putrefactive and excrementitious matter, which had been collected during former voyages among her ballast, when she had been employed as a transport ship; many of the men engaged in cleansing her were seized with yellow fever and hemorrhages; the vessel was only imperfectly cleansed, when she sailed for the bay of Honduras; on her voyage, and after her arrival there, she lost many of her crew with the yellow fever, but none of the inhabitants suffered by the disease, except those who came on board the vessel.

2°. The mode in which a yellow fever epidemic spreads and extends itself over certain localities, elucidates its infectious nature.—The disease usually remains confined

for some time to the spot where it has originated, then extends through the low, moist, and uncleanly districts; is often arrested by public squares and wide streets; and if it infects and passes those places, it is only when the infecting causes have arrived at their maximum of intensity, so as to vitiate the atmosphere of a town generally. If a person labouring under this fever be removed into a healthy district, none of those who approach him receive any disease; all which circumstances are widely different from those which attend the introduction of contagion in a populous city, where the disease spreads equally and with a rapidity in all directions, which baffles every effort to trace its progress.

3°. The prevalence of the yellow fever is sensibly controlled by meteorological phenomena.—Cold will abate its fury, or suddenly check its progress, as will also an abundant fall of rain, high winds, or a heat which shall be so great as to deprive the decaying materials of the requisite moisture to continue the putrefactive process; whilst moderate rains, hot sultry weather, and high heat, aggravate the epidemic. It has been remarked at Philadelphia, St. Domingo, and St. Lucia, that the disease was much mitigated by removing the sick from the infected district into a purer atmosphere. They have long been in the habit at St. Lucia, of taking the sick to a neighbouring hill, famed for its salubrity, where the fever has never been known to originate, or be communicated, notwithstanding the great number of sick constantly removed there. These circumstances precisely appertain to all infectious fevers, and have never been considered applicable to those propagated by contagion.

4°. The subsidence of an epidemic is always in an inverted direction to its extension.—The disease first retires from the elevated and airy districts, and last of all from the low and filthy places where it commenced, which is in perfect accordance with its supposed origin.

Finally, the concourse and progress of the symptoms,

the organic lesions, shown on autopsic examination, the first stage of inflammation, quickly succeeded by malignant symptoms, and the alarming collapse of the system, the yellowness of the surface, the black vomiting, and other ataxic phenomena, indicate the marked affinity of yellow fever with the other infectious fevers, and their maximum grade of violence.

After having thus, by clear positive fact, and close deductive reasoning, applied to every possible view of the subject, satisfactorily and conclusively substantiated the infectious character of yellow fever, Dr. Devèze proceeds to examine if, under any circumstances, it is, as it were, *accidentally* contagious.

From a review of facts, as they have been developed in those places where this fever raged with most violence, and an analytical investigation of those circumstances which have been chiefly depended on, by the apologists for its contagious character; he concludes that *it is not so*, under any circumstances. The length to which this article is extended, will prevent us from doing more, than merely mentioning very briefly the reasons on which he founds this opinion. The islands St. Lucia, Martinique, and St. Domingo, have always been considered among the most unhealthy of the West Indian archipelago; yet the former has an islet situated in one of its harbours, as we have before mentioned, where the fever never originates or extends, although it is emphatically the asylum of all the sick with this fever, who can be conveniently removed there; and the other two, contain districts equally healthy, where the inhabitants are never known to suffer from fever, and where even strangers may reside with perfect security. Lind has made the same observation, speaking of Monk's Hill in Antigua, and Bally, of Spanish-town, Jamaica. In the temporary fever hospital erected on Bush Hill, near Philadelphia, in 1793, not a nurse or assistant, or patient labouring under some other disease, and lodged in the same apartment with the sick with yellow fever, suffered from such exposure. Of the great number of sick removed into

the neighbouring country, and of persons attacked after flying from the sources of danger, not one instance was shown of the disease being communicated to the hundreds, who maintained a fearless and constant intercourse with them. The impossibility of receiving the disease by inoculation, by breathing the vapour of black vomiting, and taking internally large quantities of the inspissated material, is also insisted on as utterly repugnant to the idea of its contagious nature.

The remaining part of the work is devoted to the treatment of the disease, and recommending those preventive measures, which are calculated to mitigate or ward off the recurrence of these epidemics. In the first, or inflammatory stage, he recommends moderate and repeated venesection, with diluent drinks, warm baths, enemata, &c. In the second, he employs laxatives, blisters, sudorifics, particularly camphor combined with nitre, and moderate tonics. His prophylactic measures are the same as those which have been long ago recommended by Rush, Miller, and other American physicians; but, which we regret to say, a mistaken economy, and the spirit of party, have prevented from being adopted as fully, as could be wished.

It would be an interesting employment to follow out the results to be deduced from the doctrines as developed in this work, in order to the establishment of health laws, and a system of quarantine, which, while they should be effectual in securing their desired benefits, should have no uselessly embarrassing regulations.—But our limits at present will not allow us to enter on this interesting subject, and we defer it to some future occasion.

In taking leave of our author, who has afforded us pleasure as well as instruction, we earnestly recommend his work to the attentive perusal of such as are still vacilating in their opinion of the cause, and mode of propagation of yellow fever; in the full persuasion that it will go far to remove their doubts, providing it be read with a due regard to the apothegm of Seneca, *Pars sanitatis velle sanari fuit.*

Nosologie Naturelle, ou les Maladies du Corps humain destribuées par familles. Par J. L. Alibert, Chevalier de plusieurs ordres, Medicin consultant du Roi, Medicin de l'Hopital de St. Louis, &c. &c. &c.

Natural Nosology, or the Diseases of the Human Body, distributed into families, by J. L. Alibert, Knight of several orders, Consulting Physician to the King, Physician to the Hospital of St. Louis. Large elephant, 4to. with superbly coloured plates. Crapelet. Paris, 1817.

Condillac justly estimated the importance of nomenclature, when he said that the sciences are only well defined languages; and Locke happily defined a system to be simply an arrangement of facts in a convenient order for the memory. These truths have been overlooked alike by those who have decried nosology, and by those who have considered the solution of the theorem, "a disease being given to determine its true character, and the place it should hold in a system of nosological arrangement," the great object which the physician should ever seek to attain; as if the ultimate end of medicine were to arrange diseases, not to cure them. So far was this error carried towards the close of the last century, that we cannot be surprised if the entire rejection of nosology, commenced by men of bold and original conceptions, found many followers among physicians of sense and sound practical views. Our illustrious countryman, Rush, inculcated on his pupils a very general disregard of nosology, which we think is greatly to be commended. He has been called, and in many points of view he deserves to be considered, the American Sydenham. Yet Sydenham laid the foundation of nosology, and Rush spent his time in overturning its superstructure. When the subject is considered dispassionately, it appears unwise not to study diseases after an arrangement of them convenient for the memory. Yet nosology is nothing more: the word has long ceased to

convey its just etymological import, and might very properly be replaced by the term nosography, from which it is as widely different as geology is from geography.

The objects of nosography being to facilitate the study of diseases, it is evident that the principal merit of every arrangement must consist in the exact and invariable distinction of the species. This may appear, at first view, an easy task; yet writers have differed from each other in the number and character of their species, more than in other parts of their systems. Thus to state the difference as regards the numbers only—Sauvages has made about 800 species, Cullen 600, Sagar 2500. This shows great variety of opinion, if not great error, among some or all these writers.

But error in the definition of species is of more consequence. We shall name one, among many, sufficiently striking, and whose only claim to existence is its having legitimately descended from Sauvages to the very last of those who have selected the dross of old systems to re-mould into new. A specific distinction is made between confluent and distinct small pox, between which the only difference is the greater violence of the former. They produce each other reciprocally, the virus is the same; their origin, progress, and decline follow the same laws. Greater or less severity of symptoms forms no just foundation of specific distinction—Thus much *currente calamo*. Now to the work under consideration. M. Alibert has grouped diseases according to their seat, not after the Celsian method, *a capite ad calcem*, but in relation to the function of the part they occupy. His first class comprehends diseases of the organs of assimilation; his second of the organs of relation, and the last of the organs of generation. The first volume of his work, which alone has been published, is confined to the first class.

In treating of gastric diseases, for so M. Alibert denominates his first family, he traces with a masterly hand the various organic and functional diseases of the great

centre of the repairs of the animal machine,—a machine, which, like the bark of the Argonauts, undergoes a change in all its constituent parts before the voyage of life is ended.

The first genus of this family M. Alibert terms Pylorexia, a morbid condition of the stomach, which tends to excessive eating, without satisfying the appetite. It is attended with lypothemia, languor, pain in the stomach, defective nutrition, and emaciation. Its species are, 1. P. Bovina, in which the food is retained; 2. P. Canina, in which the food is rejected by vomiting; 3. P. Lupina, in which the food is quickly discharged by stool. We think it unfortunate that the learned author has taken it for granted, and indeed formed this genus upon the presumption that the sensation of hunger, or to speak more correctly, the disposition to take food, originates in the stomach. A case related by Morton, of an opening into the thoracic duct, which was followed by an insatiable appetite, and another of the like nature by Morgagni, render this point at least very doubtful, and leave the subject incapable of explanation in the present state of our knowledge, unless by aid of some such convenient machinery as an anima medica, archæus, or vis medicatrix naturæ.

The second genus, Heteroxia, or depraved appetite, comprehends, 1. H. Picacea, or eaters of charcoal, lime, chalk, &c. 2. H. Malacea, or eaters of things good in themselves, in improper quantities, such as pepper, vinegar, &c.—The third genus, Dysorexia, is an essential disease produced by inappetency of the stomach for solid food. This genus comprehends, 1. D. Saburralis. This arises from a surcharge of crudities and suburral matter, which diminishes the natural contractile power of the stomach. The symptoms are nausea, with a sense of satiety and repletion. 2. D. Peculiaris, a repugnance to particular climates. 3. D. Spasmodica, affecting particularly nervous persons.—The fourth genus, Polydepsia, a disposition to take improper quantities of liquids. This has two species,

P. Hydrophota, and P. Vinosa.—Genus the fifth, Adipsia, has two species, 1. A. Primaria. 2. A. Nervosa.—The sixth genus, Dyspepsia. M. Alibert enumerates under this genus four species; 1. D. Biliosa. 2. D. Mucosa. 3. D. Nidorosa. 4. D. Flatulenta.

M. Alibert thus characterizes this genus: "Dyspeptic persons experience a weight in the stomach as soon as they have taken food. A long time afterwards this organ is fatigued by an inconvenient sense of fulness. Digestion is carried on with so much difficulty, that even on the following day there is nausea, pain in the stomach, tension, painful swelling of the epigastrium, and secretions of various kinds, according to the nature of the case, and the food last taken. The patients are uneasy, languid, and morose, incapable of occupation, and martyrs to constipation. When the stomach is weak, and the ingesta are incapable of digestion, they undergo a sort of fermentation, not unlike what takes place out of the body. It is in fact a chemical process, and the substances become decomposed and putrid.

"The most manifest and common symptoms are painful tension of the stomach, a sense of burning, frequently accompanied with acid or bitter eructations. The patients frequently exhale an odour of rotten eggs, or of empyreuma or rancid oil, and other smells not well defined; as for instance, that of Parmasan cheese. The alkaline odour of the bile is not less insupportable to those who come near the sick patients. There are cases in which the organs of digestion are coated with a viscid paste, or by bitter and putrescent food, from which arises a general uneasiness of the animal economy. An intolerable pain is felt in the epigastric region, and headach, sometimes very severe and constant, and often accompanied with vertigo, like that which causes a drunken man to stagger. There is cardialgia, salivation, and burning thirst. The stomach relieves itself by vomiting, or the nausea continues some hours after the meals.

"The flatulent swelling of the stomach and intestines,

occasions spasms and distress. Sometimes the vires medicatrices naturæ bring on a salutary diarrhœa; more commonly there is a constipation and a colic. The circulation is so slow, that the heart scarcely throws the blood to the skin. The blood accumulates in the capillary vessels, and the face swollen, puffed up and livid, presents a singular aspect. The languor of the circulation also causes the skin to be dry, contracted, and cold, especially in the extremities. The muscles are weakened, and their movements are indicative of lassitude and heaviness. Although fatigued, the patient cannot sleep; but is disturbed by frightful dreams, which cause him to start up suddenly. The mind is not less affected. The ideas have no connexion, no order; the faculty of reflection is impaired, and the temper is morose and irascible. Some individuals become sad and despondent. The slightest shocks, fear, and similar affections, bring on universal tremblings; others fall into a state of profound apathy.

"In the most aggravated forms of dyspepsia the pulse is wiery and contracted. The chest is continually oppressed, and a sense of suffocation occurs. The intestines are affected with lancinating pains, and the body is covered with a cold sweat. Some patients die with well marked symptoms of apoplexy; others with hectic fever; anasarca and dropsy precede death. In some cases the stomach becomes uncommonly distended, by the evolution of gases, and its two orifices are totally closed. The patients then attempt in vain to free themselves from the anguish attending this occurrence, by vomiting. They expire in dreadful agony.—A dyspeptic soldier, who was occasionally voracious, ate largely of bread fresh from the oven. It swelled like a sponge in his stomach, and he died almost immediately." P. 26. & seq.

"The Entirorrheas," [Diarrhœas] says M. Alibert, "we have had occasion to observe among our soldiers, came on after long fatigues and privations. They were very obstinate. The patients complained of contusing pains in all

their limbs; walking even in the wards aggravated all the symptoms. It was not until after several days of repose that the stools became a little more consistent. The stomach, unaccustomed to improper food, seemed as if drawn in opposite directions. The chest was oppressed, and the nights were passed in dreams or disturbed sleep. Melioration after treatment was often deceptive. The disease returned with renewed violence. The countenance of the patient was pale, clayey, and the features sharpened: the pulse small, frequent, especially towards evening; the hands and arms were swollen, and showed scorbutic spots. Most of these diarrhœas arose from chronic inflammation of the mucous membrane of the intestinal canal, and in all cases the marks of inflammation were most striking in the colon, the internal surface of which was of a deep red, even when the inflammation extended over all the canal. In one case of colliquative diarrhœa, the colon appeared as if lacerated in several places." P. 73, 74.

"By Pneumonalgia, [Angina Pectoris,] M. Alibert designates two species—P. Idiopathica, and P. Symptomatica. The former of which he holds to consist in an essential affection of the pulmonic nerves, and the latter to depend upon ossification at the coronary arteries, hydrops pericardii, &c. The pain in the sternum, which is regarded as the pathagnomonic sign of pneumonalgia, although most frequently on the left side, may also extend to the right pap, and it often happens that the scrobiculus cordis is affected. Finally, under some circumstances the central pain ceases, and the fingers of one or both hands are benumbed, or are affected with a sense of pricking, which is commonly a bad omen. The fever which accompanies this disease is of a very irregular type: the skin is dry and hot, or covered with clammy sweat; the urine is pale and scanty." P. 266.

Again, "To resume the enumeration of the principal symptoms of Pneumonalgia. The disease consists of a very painful and severe oppression, principally in the mid-

dle of the sternum, coming on at intervals, and by paroxysms. The patient complains of a painful sensation under the left pap. Sometimes the arm of the same side is benumbed, and the pulse is hard and irregular. These symptoms are followed by vertigo, dimness of sight, fainting, frequent efforts to cough, convulsive motions of the chest. The chest is compressed as if in a vice, the face is sometimes extremely pale, at others red and inflamed. The horizontal position is insupportable, the patient jumps from his bed, and discovers extraordinary agitation."—P. 267.

Mr. Jurine, of Geneva, who has recently obtained the prize medal of the Medical Faculty of Paris, for the best dissertation on Angina Pectoris, very aptly compares its symptoms to those produced by ascending a high mountain, and persisting in this severe exercise until exhaustion ensues. M. Alibert does not notice the opinion of an American professor, that the disease is owing to an accumulation of blood about the centre of the circulation.

The inconvenience of this system is, that as it is necessary to treat very different diseases under the same head, many unfortunate alliances, and unnatural separations occur. Though the hand of a master is discoverable in every line, the reader is driven to the humiliating conclusion, that the ingenuity of man will never devise a perfect system of nosography, nor even the most perfect that is attainable, founded upon any one part of pathology. But that, on the contrary, the most unobjectionable classification will be the result of a discreet observation of the symptoms of diseases in some cases; of their causes and their seats in others.

Jussieu's arrangement of plants according to their natural orders and classes, was not founded upon their relations to each other, as regarded the different soils in which they grew, but upon some fundamental and constant characters of the plants themselves; and of genera, composed of individuals, having an affinity to each other. But what affinity is there between Bulimia and Gastritis? and

between either of these and schirrus of the pylorus. M. Alibert has admitted as essential diseases, affections which have long since been acknowledged to be mere symptoms, and this is unquestionably a capital error in a system of nosography. Yet in our opinion he has more than compensated for this, by the profound manner in which he has treated of them. Etiology has been so much neglected, that we are not displeased to see it find a place in a work avowedly devoted to a different object. The descriptions of individual diseases in M. Alibert's book, are drawn in an easy, pleasing style, abounding in important and apposite facts, which cannot fail to make it singularly valuable.

Observations on Febrile Contagion, and on the means of improving the Medical Police of the City of New-York. Delivered as an Introductory Discourse, in the Hall of the College of Physicians and Surgeons, on the Sixth day of November, 1820. *By* David Hosack, *M. D. Resident Physician for the City of New-York; Professor of the Institutes and Practice of Medicine in the University, &c. &c.* N. York, Bliss. *pp.* 51. *With an Appendix.* 1820.

It is not often that we have an opportunity of making introductory lectures the subject of our remarks. They are usually considered by the authors, rather in the light of required exercises, with which public teachers cannot with propriety dispense, although they may be often found inconvenient, than as regular philosophical essays, claiming public attention on account of the value of their subject matter. They were wont, when we were young, to be looked upon as bills of fare, which the professor thought it his duty to present to his hearers, who, for the most part, are students, in order that they might form some judgment of the kind and quality of the materials which they would

be required to digest from day to day during the course; they therefore could not possess, from their variety, either of incident or illustration, sufficient novelty to render their publication either profitable or proper. Believing, as we do, that their publication, if more generally adopted, would essentially serve the great purposes of medical science, by enabling the community to judge of the qualifications of public teachers, we cannot but regret the want of some authoritative rule in our colleges on this subject. They are the only occasions which the professor possesses of establishing his claims to public confidence; and they, in return, furnish the only opportunities to test his merits for the station which his talents, his acquirements, his friends, or peradventure accident, has permitted him to occupy. It is true, it would subject him to appear once in every year before the public; but feeling a necessity which he could not control, his genius and his industry would be put in requisition to satisfy expectation, however extravagant; his indolence, his self-sufficiency, and his vanity, if he has any, would occasionally receive a salutary check; and the collisions of opinion in matters purely speculative, would teach him that diffidence, or rather tempered confidence, which, although such necessary ingredients in the character of the man of science, his avocation as a teacher of youth, is continually calculated to destroy.

After an opinion so decidedly expressed, it would be superfluous in us to confess our satisfaction with the conduct of the Professor of the Practice of Physic, who has fearlessly encountered public opinion, and challenged public scrutiny, by printing the lecture whose title stands at the head of this article; but lest we may be misunderstood, we mean to speak solely with reference to its publication, hoping that in future it may serve as a precedent—As for the spirit in which it is written, the soundness of the reasoning it contains, and the motives which influenced the writer, so far as they may be gathered from the pamphlet,

these, we think, are fairly the subjects of remark, to which we intend to devote some half dozen pages.

Yellow fever has so long been a topic of discussion among physicians, the facts are so numerous, and the arguments have been so frequently urged, that it appears at this day a thing next to impossible to create any other interest on this subject, than that which is in some way or other connected with the terror which the disease inspires, and the distress which it necessarily occasions. To an inquirer after truth, the only difficulty would seem to be in generalizing the facts, for when once arranged with their attending circumstances, the reasonings must be short and conclusive. Many of the advocates for its personal communicability, aware of this, have done all in their power to prevent the adoption of terms, upon which it would be impossible to mistake; and if not mistaken, it would be as impossible to reason long without arriving at a conclusion.

The author of the discourse, of which we are now about to speak, is of the number of those, who, to a pertinacity of opinion which is scarcely qualified by an awkward attempt at compromise, adds the affectation of a most superlative contempt for the doctrines of those who differ from him, and scarcely permits himself to believe, that any thing which they have seriously urged, is entitled to be seriously considered. How far his reasoning has succeeded in making the like impression on his readers, will best appear from the pamphlet itself.

After some general observations, inviting their attention, &c. to the remarks which are to follow, he states the differences of opinion which exist on the subject of yellow fever in the following manner:

"You are doubtless aware of the opposite opinions which have divided the physicians of the United States relative to the contagious nature of yellow fever, and of the controversies to which that question has given birth, relative to the domestic origin or foreign source of that disease. The same diversity of opinion, though in a less degree, exists among the physicians

of the West Indies, and of Europe ; and, latterly, in this country, the same question has been agitated relative to the contagiousness of dysentery, the various forms of typhus, jail, ship, or hospital fever, and has been extended even to the plague itself: each too contends that his opinion is legitimately deduced from facts. Paradoxical as it may seem, this is truly the case; they are both right, and each reasons correctly from the premises he assumes : for example, he who contends that the yellow fever is contagious, under all circumstances, in the pure air of the country, as well as in the foul air of the city, certainly contradicts the facts which repeated observations has established. On the other hand, he who denies its contagiousness in a foul atmosphere, in the confined and crowded dwellings of the poor, in the impure air of our cities, in the vicinity of the water side, where the materials producing such impurities are most abundant, no less violates the truth."—pp. 4, 5.

Here we find our author asserting the consistency of those who hold opposing opinions, and maintaining that each reasons correctly from the premises which he assumes, though he leaves us to conjecture what these premises are. That this may be the fact with respect to yellow fever, as well as every other contested question, we have no doubt ; but that we can judge of their correct reasoning, without a knowledge of the data upon which it is founded, appears to us rather difficult, particularly as in this case, the example which the writer adduces, in place of proving that they are both *right*, shows conclusively that they are both *wrong*.

But, taking it for granted that he believes what he has written, to wit, that they *reasonably* differ, we cannot but view the acknowledgment as most unfortunate for the Doctor; since, to the candid reader, the largest half of this lecture will appear to be, as it in reality is, an illiberal and libellous attack on a vast majority of the physicians of this country ; and he has taken care that the scope of it should not be mistaken, by confessing also, that it is gratuitous and wanton. His reason for this (in this

case *unreasonable* concession,) may perhaps be founded on the disappointment which has followed his ineffectual attempt "to reconcile opposing opinions and explain contradictory results;" if so, we are glad of it, for conduct like this requires some apology.

"I began the inquiry," says the professor, "by asking the question, what is a contagious disease? To this question I found no answer had been given, in which physicians were united. Many had been proposed according to the particular disease each had happened to select as the standard of comparison; for example, the small-pox was by some made the standard. This disease is communicated by contact, by the air, and can be contracted but once in the course of life. Yellow fever, and many other febrile diseases, usually deemed contagious, referred to this as the exemplar, are certainly not contagious diseases; but those who reason thus, and deny the contagious nature of yellow fever and plague, because they may be taken more than once, forget that there are also diseases, on all hands admitted to be contagious, which can be repeatedly contracted by the same person, as the itch and the venereal disease."—p. 5.

That he should have commenced his inquiry by asking what was a contagious disease, in order to inform himself before he presumed *tantas componere lites*, we think was perfectly rational; the marvel is, that he should have forgotten to give us the answer; for it would puzzle a casuist to gather, from this lecture, what the peculiar tenets of the professor are.

When an author, as in the present case, has no particular text to follow, and is not arguing with an antagonist, but is at liberty to make out his own cases, and answer objections of his own raising; it ought to be presumed that his reasoning would be fair, and his deductions logical: speaking of the small-pox he says, "this disease can be contracted but once in the course of life," evidently referring to this as the most characteristic difference between it and yellow fever; on which account this last cannot be conta-

gious, if small-pox be made the criterion by which we are to judge; and, as if to show that this concession might have been withheld, we are gravely told, that gentlemen who reason thus, and deny the contagiousness of yellow fever, because it may be taken more than once, forget that there are also diseases, on all hands admitted to be contagious, which can be repeatedly contracted by the same person; and he instances itch and syphilis, neither of which are febrile diseases—and he is discussing the subject of *febrile* contagion! Now we undertake to say, that no man, in the possession of his senses, ever maintained that it was the character of contagious diseases, that they never affected the constitution more than once; but many have contended, and the position has not yet been successfully controverted, that *it is* the character of all *febrile contagions*. Having disposed of this point thus summarily, he proceeds,

"Driven from this ground as untenable, a distinction was resorted to, that some diseases are contagious and others infectious. What then, I ask, is an infectious disease, as distinguished from one that is contagious? An infectious disease, they allege, is not derived from the sick body itself, but from the foul air in which the sick person happens to be lodged: but they have never told us how it happens that the foul air surrounding the sick, always possesses the wonderful property of communicating, not invariably the same disease, as would be expected if proceeding from foul air alone, but precisely that disease under which the patient happens at that time to labour. Is he sick with yellow fever?—the by-stander gets yellow fever. Is he ill of dysentery?—dysentery is in like manner communicated: therefore, I contend, that the disease, as well as the atmosphere surrounding the sick, has something to do in the communication of the disorder in question, but which has been strangely and totally overlooked by almost every writer on this subject,"—pp. 5, 6.

Here we have exhibited another evidence of the author's loose manner of reasoning, which has not even the inge-

nuity of a sophism to palliate the fallacy of the conclusion. Non-Contagionists *do* say, that the difference between *contagion* and *infection* consists mainly in this; that the one is formed within the body, and is the result of morbid secretion, and the other is formed without, and is the result of the combined action of heat, moisture, and putrescible materials, forming an atmosphere capable of producing disease. But how happens it, says the professor, that this foul air surrounding the sick, does not invariably produce the same disease, as would be expected, (by the Doctor only, we presume,) if foul air alone were the cause? This foul air must possess a wonderful property: it is operative to the injury of health and life in more ways than one! It is even so, Doctor; and really, we can perceive nothing very wonderful in this! we know that the foul air of marshes will produce *intermittents* in the spring, *dysenteries* in summer, and *cholera* and *bilious remittents* in autumn; and that its action is modified by occasional causes, which favour or control its operation—We know that the foul air of a dissecting-room will produce dysentery, and the foul air of an hospital, typhus; and being familiar with these different effects of foul air, why should we wonder? Neither is the conclusion which is drawn from this "wonderful property" of foul air, more correct than his assumed premises: it is *not* true, that if the patient is sick with yellow fever, the by-stander gets yellow fever; if with dysentery, he gets dysentery; for neither in the one case nor in the other will he get either, once in a hundred times, if he is in health; and if he is predisposed to disease, or already sick, his disease will most frequently partake of the nature of this modifying cause; but even allowing his reasoning to be unobjectionable, there certainly can be no necessity for his *contending* for that which no reasonable man ever called in question; and if it be not altogether superfluous, it appears to us not very easily reconcileable with the position laid down in his 28th page, "*that the solar heat of the tropics,*

long continued, and acting on the northern man, is the exclusive source of yellow fever.

The author next proceeds to detail the substance of his letter to Dr. Chisholm, written in 1808;* which contains the celebrated attempt at the reconciliation of the different and discordant opinions on this important subject; and after entering a caveat against the imputation of vanity, which, by the way, though we do not absolutely pronounce upon, seems to imply a necessity which, but for the remark, might have passed unobserved, arraigns the good sense, the capacity, the practical information, and even the integrity of all such as have the misfortune not to think as he does of the aforesaid paper. We shall not detain ourselves to remark upon the extraordinary character of this letter, which has been before the public twelve years; nor upon that "wonderful" *sagesse* which, whilst it spends its energies in maintaining a peculiar theory, unconsciously contends for the correctness of disputed facts, which, if admitted, destroy the foundation on which the doctrine is built. That an author should be greatly enamoured of his own opinions, is natural enough, and withal very harmless; but that every reader should be called a fool who does not feel the same partiality as the writer, appears to us not only unreasonable, but uncourteous; and if it be not impudent, it at least savours of what the Doctor in this lecture calls "overweening conceit:"—"latterly," he says, "however, the distribution proposed, and the principles connected with it, have been called in question by some of the medical gentlemen in this country, who, it appears, have not had an opportunity, either from reading or practical observation, of knowing the facts from which these deductions have been made, to which I have referred. They have,

* Great pains have been taken to render this letter as public as the "noonday sun, earth, air, or water, or any common benefit of nature:" it may be found in every form, from the ponderous quarto of our Philosophical Society, down to the "three-penny sheet of a daily news-letter." Whether the revenue of reputation be commensurate with the labour of distribution, the professor can best judge.

from the want of this knowledge, precipitately run into the absolute and unqualified disbelief, not only of the correctness of the [my] view taken of this subject, but of the contagiousness of the fevers enumerated under any circumstances." Without stopping to inquire into the truth of this imputation, we will only observe, that we know not to whom his last observation applies. The reading of the author is no doubt more extensive than our own; and perhaps he may have authority for this assertion; we merely acknowledge our ignorance on this point, and hope that in the next edition he will be a little more particular, and give us the names. But to proceed—

"I furthermore ask, are the unfledged opinions and speculations of those men of the closet, who yet have had but few opportunities to test them at the bed-side, to come in competition with the results of experience, aided by the most profound professional learning, and the distinguished abilities of the numerous writers who have adorned the past, and many of whom still continue to enlighten the present age?"—p. 8.

"Are the investigations and experience of Huxham, Haygarth, Currie"—and here follows a host of names, who are made to play the same distinguished part as nominal referees on this long-contested question, they have been required to play a thousand times before—"to be prostrated by *the arrogant assertions, the overweening conceits, and flippant remarks of those juniors in knowledge and in years, who have lately obtruded themselves upon the public*, &c." and having premised that they have no claim to a serious and laboured refutation, [which no doubt is the author's reason for not undertaking it] he goes on to particularize four points of difference between himself and those who hold opposite opinions; at the same time remarking, that some late writers in Boston, Baltimore, Philadelphia, and in this city, who have considered yellow fever, and the bilious remittent of warm seasons, as different grades of the same complaint, "have *disgraced* the medical character, and

evinced a total ignorance of the diagnostics of disease." That there are writers in all these places, and throughout America, as well as in Europe and the West Indies, and that they are very numerous and respectable, who hold this doctrine, we know full well; but that they have disgraced their profession, and betrayed a total ignorance of all the diagnostics of disease, we do not yet believe; and we hope that we may be excused for withholding our assent from this calumny, so long, at least, as it remains doubtful whether the professor himself is a competent authority in this case: for the early history of the epidemic of 1819, some incidents of the summer of 1820,* and the celebrated letters to the Recorder of our city on the subject of the cases of the two Messrs. Van Nests, have had a tendency to increase our scepticism on this point, notwithstanding his experience of thirty years.†

* The case of *Alkoholic fever*, mistaken for yellow fever by Dr. H. which occurred in August last, in a subject by the name of Williams, has materially impaired our confidence in his infallibility. *Quere*, Would it not be well for some of our modern *nosologists* to settle the diagnostics of this fever, in order that the same mistake may not happen again? it might perhaps be difficult, seeing it has so many characters in common with other fevers, as to have deceived the professor himself; but it is worthy the attempt; for whether contagious or infectious, or neither, it is certainly *catching*; it is not imported, 'tis true, but its *cause*, for the most part, is undoubtedly of foreign origin; and whether pestilential or epidemic, it is on all hands allowed to be remorseless in its attacks, and destructive in its ravages; and as in the case of the above-named subject, it was cured by a visit to the Marine Hospital, by order of our author, in forty-eight hours, this circumstance is highly interesting to the public, particularly if it should be proved to have Mr. Pym's attribute of yellow fever, viz. that it never affects the same person more than once during life!

† *The following are the two Letters mentioned in the text, and we trust we shall receive the thanks of the author for rescuing them from the neglect which awaited them, from the fugitive character of a newspaper, in which they were inserted.*

Letter from Dr. Hosack to Mr. Jay, the Recorder and President pro tem. of the Board of Health.

New-York, Sept. 5, 1819.

Dear Sir—Upon the receipt of your note, accompanied by Dr. Dyckman, I immediately visited the family of Mr. Van Ness. I was informed, as

We hope that we shall not be thought tedious, if we attempt to sketch the Doctor's four points, and his summary mode of disposing of them. The facts he adduces are as

stated by you, that the nephew of Mr. Van Ness had died of fever at an early hour this morning. The body being enclosed in the coffin, ready for interment, and the friends assembling for that purpose, we could not obtain a sight of it without inconvenience, and creating a great degree of alarm.

We visited the son of Mr Van Ness in an adjoining room, lying severely ill of fever, and which will probably in a few hours prove fatal.

Was I to judge merely from the external characters of the disease, as exhibited at a single visit, I frankly acknowledge I should pronounce it a case of *yellow fever*. But upon conversing with one of the physicians in attendance, and obtaining from him a circumstantial account of the commencement and progress of the disease, I find that some of the essential features of that form of fever have been wanting in both these cases, and that the disease has exhibited throughout the symptoms of *bilious remittent fever*. At all events it has been uncommonly rapid in its progress, and is attended with an unusual degree of malignancy; and as all *continued fevers* at this season of the year are liable to be communicated, I should recommend that measures be taken to have the apartments the sick have occupied, with their clothes, beds and bedding, immediately cleansed and ventilated.

I am, Dear Sir, respectfully your's,

DAVID HOSACK.

The Hon. Peter A. Jay, Recorder.

Additional Letter from Dr. Hosack to the Recorder.

New-York, Sept. 6, 1819.

Dear Sir—The short period of time between the receipt of your note yesterday afternoon and the meeting of the Board of Health, at five o'clock, did not admit of so full and explicit a reply as I wished to have communicated, or as the importance of the subject demanded.

I stated to you yesterday, in general terms, that, from the information derived from the attending physician, *some* of the essential symptoms constituting yellow fever appeared to have been wanting in both cases referred to; and I add, from the same source, that the sick exhibited other symptoms not usually attendant upon the *yellow fever*, which this has been unequivocally so denominated by the resident physician.

In the first place, I remark, upon the authority of Dr Boyd, the attending physician, that the disease in both of those cases was ushered in by *chills*—this is very generally the case in the *bilious remitting*, but is of less frequent occurrence in the commencement of *yellow fever*.

2d. The eyes did not exhibit the *inordinate redness* that announces the attack of *yellow* fever—in these cases this symptom did not appear until three or four days had elapsed.

familiar to the reader, as the very name of the doctrine which they are intended to support; and his authorities, which are numerous, can, in the present state of this question, serve little other purpose, than to confound the inquirer after truth, seeing that there are scarcely any two of them whose views fully accord.

"In the first place, they (non-contagionists) deny the peculiar character of yellow fever as distinct from the or-

3d. In the first of these cases no yellowness appeared upon the skin until after death—on the contrary, the whiteness of the skin was particularly observed by Dr Boyd upon the morning of the patient's decease. But, in the case we visited yesterday, the yellowness of the skin was very generally diffused.

4th. Neither of the patients had the *black vomit*, which very generally accompanies, or soon succeeds, the yellowness of skin in yellow fever, and like the last mentioned symptom, commonly appears between the third and fifth days of the disease.

5th. Upon the exhibition of an emetic in the commencement of the disease, the bile was freely discharged by vomiting; this is not usually the case in yellow fever—on the contrary, the bilious secretion is sensibly diminished in yellow fever, but is very abundant in the bilious remittent

6th. Upon the exhibition of rhubarb and magnesia, to use the language of Dr. Boyd, "the bowels readily responded to the medicine prescribed;" whereas in yellow fever there is more or less of torpor in the intestines, and more active medicines are called for.

7th. The pulse of the patient we visited yesterday amounted to 115 in a minute, ascertained by one of the physicians present. Whereas, in yellow fever, at the same period of the disease, the pulse is less frequent than natural, and oftentimes full and soft, almost constituting the pulse of health instead of disease

8th. Upon examining the tongue of the patient, it was furred over its whole surface: this is an unusual circumstance in *yellow fever*. On the contrary, in this form of fever, it is frequently clean and moist, and, in correspondence with these appearances, the patient is oftentimes without thirst throughout every stage of the disease. Dr. Boyd also added, that the whole progress of the fever, in both of these cases, manifested the marks of the common bilious remittent, such as he had been accustomed to see for many years past, both in town and country.

In these details you perceive the grounds upon which I expressed to you the opinion that these are probably cases of *bilious remitting fever*, which the first view of the subject might have led me to suppose.

I am, Dear Sir, respectfully your's,

DAVID HOSACK.

The Hon. Peter A. Jay, Esq. Recorder.

dinary bilious and typhus fevers of our country, considering them as grades of the same disease." In thus stating this point of difference, it is evident, that he affects to consider all non-contagionists as persons holding the doctrine of the *unity* of fever; which position, whether true or false, although it has nothing to do with the previous question which he has purposed to discuss, he very seriously impugns, in very dense, though not very decorous language, referring us to his own experience to disprove the doctrine, and lays it at rest, by roundly asserting, that this form of fever, viz. bilious remittent, he has prescribed for in mid-winter; and moreover, that he has, within a very few days, and since the appearance of frost, seen it. The reader, upon this single assertion, is required to renounce doctrines, which have been maintained by the most illustrious medical talent in our country; and in order to make this testimony serve, the author feels constrained to remark, that gentlemen who hold these opinions are persons of slender information, and when they shall have read the pages of Lind, Pringle, and others, they will wish that their publications had never seen the light—But as we feel our inability to do justice to this part of his essay, we will transcribe his own words, and leave the reader to form his own judgment.

"When the writers referred to shall have attentively perused the pages of Lind, Pringle, Blane, Lempriere, Niell, and Pym, who have most abundantly drawn the distinguishing characters of bilious and yellow fever, and whose diagnostics are derived from the bedside of the sick, not the abstract productions of the writing desk, they will wish that the publications to which they have affixed their names, had never seen the light. Indeed, as it regards their knowledge of yellow fever, I feel constrained to remark, that it would almost appear that one of the great qualifications of those gentlemen is to be practically unacquainted with the disease on which they write; and, as in viewing certain objects, it is necessary for the observer, for the purpose of more distinct vision, to withdraw to

a considerable distance from the object to be inspected, they, in like manner, have considered it equally proper to take a distant view of the epidemic they are to describe. Such I allege has been the fact, that the great number of those who have written most, and in the most dogmatic style, not only in the earlier years, when yellow fever visited our shores, but in the later seasons of its appearance, have immediately, upon the commencement of the disease, fled to a distance from the scene of distress:—but what they want in facts they supply by bold assertion, and for dispassionate reasoning they substitute the coarsest personalities."—p. 11.

For what earthly purpose the professor lugged in the last sentence of this paragraph, which is not true in fact, and which if true, is discreditable to him, as evincing an envious and detractive spirit, we are at a loss to conjecture, unless perhaps it was to fill out the picture which he had drawn of our physicians, that if it had not the merit of a likeness, it would serve as an admirable caricature. After some further observations in reference to Dr. Bancroft, whom he represents as not having learned his horn-book, though it is fair to presume, that he has been subjected to the teachings of Lind, Haygarth, and Lucretius, he says,

"I contend, that as the yellow fever did not simultaneously appear in different parts of the town, but successively, beginning at the water-side, in the immediate vicinity of a tainted ship; and as in all the subsequent cases they were traced by the Board of Health to the place of its first appearance, the inference is certainly adverse to the domestic origin of the disease, and in favour of its introduction from abroad. I know not what is truth or correct reasoning, if any other deduction is admissible from the premises that have been stated."—pp. 12, 13.

We know not in what terms to notice the above quotation. It was our intention to remark upon this extraordinary lecture in terms that should at least be respectful; but here we find a difficulty which we did not apprehend; the truth must be our apology, though it may be offensive. If

the author meant to state the facts hypothetically: if his printer has mistaken the word *if* in his manuscript for *as*, we shall not quarrel with his inference; but if there be no typographical error, then we challenge the professor to the proof of what he has here asserted, asking no more of the reader, than that he should consider it as resting solely on the writer's authority. As for ourselves, we are content with its flat denial; for we *know* it to be not only unsupported by testimony, but contradicted by the most respectable authority.*

The Doctor's second point is, that "they (meaning the writers of whom he has before spoken *so* respectfully) deny the contagious character of the disease, and thereby come in conflict with the most distinguished men of our profession." This is true, at least in part; and while we are happy to bear our testimony to the merits of the writers whom he enumerates as the most distinguished, we may also be permitted to remark, that very few of them agree on the laws which govern this contagion, as they style it, and all of them have purposely overlooked the plain and obvious distinction between the terms Contagion and Infection. Some believe it to be but once received in the course of life, thereby identifying it with small pox in its most distinctive character: others, that it is an endemic nowhere: others again, that it is endemic in tropical climates; and we believe all of them who are living agree to reject the conciliating theory of Professor Hosack himself, which claims the extraordinary merit of reconciling the doctrine of *incidental* contagiousness, with the *exclusive* importation of yellow fever, and assumes for its basis a fact, which is not only unsupported

* Vide Dr. R. Bayley's Letter to Governor Jay—Med. Rep. vol. i. p. 121.—Dr. Warren ditto to the Secretary of the American Academy of Arts and Sciences—Med. Rep. vol. i. p. 131.—Report of the Academy of Medicine of Philadelphia to Governor Mifflin, presented 1798.—Dr. Derosset's Letter to Dr. Miller—Med. Rep. vol. ii. p. 143.—Dr. Rand's Account of Yellow Fever, at Boston, 1798.—Rush's Med. Inquiries, and Med. Rep passim.

by experiment, but at variance with every chemical analogy. The limits of this article will not permit us to enter at large upon this subject, and we must refer the reader, after the example of the professor, to the works of Rush, Ramsay, Miller, Bayley, Mitchill, Pascalis, Caldwell, Pouppé Desportes, Lind. Pringle, Pinckard, Assalini, Humboldt, Bancroft, Moseley, Jackson, Hillary, Le Blond, Devèze, Savarésy, Maclean, &c. &c. &c.

The third and fourth points are, that they reject the doctrine of its importation, and contend for its domestic origin. In combating these two positions, we are presented with broad assertion on the one hand, and a reference to authorities on the other, some of whom so far from having the confidence or experience, which is so essential in discussions of this sort, were utterly destitute of both, particularly in relation to matters of fact.

"Passing over, too, (says the Doctor,) the facts which have been disclosed by the observations of Dr Lining, of Charleston; Dr. Mitchell, of Virginia; Dr. Chisholm; Dr. Dancer, of Jamaica; Dr. Stewart, of Grenada, and others, I shall only refer to the introduction and propagation of the disease in question, since the year 1800, in Cadiz, Malaga, Gibraltar, Carthagena, Alicant, and Leghorn, as illustrated in the very able report of Dr. Arejula, of Madrid, and the commissioners appointed by the French government to investigate its origin and character; and who have described its spread in those places, from house to house in succession, and to some of the neighbouring villages holding communication with them."—pp. 18, 19.

Now we undertake to say, and upon authority too which will not be questioned, (the parties themselves,) that the commission sent from Paris to Cadiz, in 1800 and 1803, in both instances arrived too late to see the disease; that the work which the first commission published on their return in 1801, was in a great measure grounded on the opinions of the Spanish physicians resident there, or derived from popular rumour; and the commission of 1803 made no positive

declaration on this subject. Dr. Arejula frankly acknowledges, in his report to the Supreme Council of Madrid, that it was impossible to know by what conveyance the infection was brought. And in respect to Malaga, it is well understood, that the vessel which in 1803 was charged with introducing the fever which desolated that city, arrived so long before the disease declared itself, (47 days,) that Dr. Bally himself, all circumstances considered, thought it much more reasonable to charge the infection to some arrival of some preceding year.

In treating of the domestic sources of yellow fever, the author preserves his accustomed tone of argumnet, stating just so much of fact as is convenient for his purpose, though sometimes, by accident we presume, a little more. He presents us, on the one hand, with the authority of Dr. Stewart, to prove that vegetable putrefaction cannot produce it; and on the other, his own experience, and that of Dr. Bayley, to show that animal putrefaction is unequal to its generation; thus having purposely separated these two processes, for which singly, as causes, few non-contagionists ever seriously contended, he proceeds to convince himself, and by the way, in that he seems to have succeeded, that yellow fever has no dependence of any sort upon any agent which they have assigned, except *heat;* and arrives at last to the conclusion, which no contagionist, though he may have hinted it, ever had the confidence to avow, to wit, "*that the solar heat, long continued, and acting on the northern man, is the exclusive source of yellow fever.*" We are not sure that the professor is in earnest here: indeed, we incline to the opinion that he is not; for the position is not more at variance with common sense, than with his own celebrated letter to Chisholm—This must be our apology for not dwelling upon it. The discovery is no doubt a happy one for all his European authorities, inasmuch as, if they can but believe it, they need give themselves no further trouble in accounting for the origin

of the disease *de novo*, which has heretofore not a little puzzled them: their ground is now emphatically narrowed to a point, and admits of no more difficulty; for so long as ships from Europe or America continue to trade in tropical latitudes, the "cause exclusive" will be obvious. Whether the reasoning which has operated this result in the doctor's mind, be just and philosophical, we leave the reader to determine; but for ourselves, we frankly declare, that we are yet among the faithless.

We would here rest on this part of the book; but we feel that we should do less than our duty enjoins, were we to omit to mention two circumstances in the above chain of argument, which, if not unfairly quoted, are certainly suspicious. In speaking of the exemption of our cities from the visits of pestilence during the time in which our commerce with the West Indies was interdicted by the embargo, he says, "A recurrence, too, to the meteorological observations of *that period*, will show a thermometrical range frequently higher than in those years when the yellow fever prevailed in our cities;" and for proof, he refers us to a note in the Appendix, which is perfectly silent on the subject of temperature in any part of America, and which, in fact, contains no information at all on temperature during the years of which he speaks. With respect to the recantation of Professor Ellicott, mentioned in the 29th page, and more fully enlarged upon in a note, we confess that we should have been better satisfied, if we had it from under his own hand, not that we would insinuate that the doctor would wilfully misquote him, far from it; but the parole testimony of an absent witness, particularly if received at second hand, is peculiarly liable to be misinterpreted: and if in this case it should be so, it is impossible to correct it. Besides, too, it must be remembered, that the testimony of Mr. Ellicott respecting the prevalence of yellow fever in certain inland parts of our country, was addressed to the public under his own signature,

many years since, and has remained uncontradicted from that time to the present.*

Having dismissed the subject of contagious fevers, our author passes, by a very easy transition, to some general remarks upon the most eligible mode of preventing or alleviating the evils which necessarily attend them. And here we are agreeably surprised to find that the tone of his language, and the entire spirit of his reasoning, is very much improved. It is by much the best part of the lecture; and although, if we except perhaps the doctor's predilection for *grave vaults* and *water closets*, there is not a single novel idea in it; yet the means recommended are for the most part of sufficient interest to claim the serious attention of those who have in charge the great concerns of our city.

The first in order is the establishment of fever wards, or houses of recovery, where the poor and necessitous may be admitted in sickly seasons, and where such of this class as may be diseased during the prevalence of any serious epidemic, might be successfully treated. The benefits which have resulted from their introduction in the great cities and manufacturing towns of England, since first recommended by Dr. Haygarth, fully warrant us in anticipating the same happy consequences in all the cities of the United States, where the causes of fever are aggravated by the evils of pauperism; and we hope that the consideration of the expense attending their erection, will not long operate to delay the experiment.

The second is the introduction of a multitude of common sewers, the benefits of which, though spoken of very

* The Doctor would have his readers believe that this fact stands recorded on the authority of Professor Ellicott alone;—but this is very far from the truth. Numerous instances will be found in the volumes of this Journal, and one of which he could not have been ignorant, seeing it is on the same page with the extract from Ellicott's Journal, and communicated to the Editors by Dr. Watkins, who was familiar with the disease, and states it from personal observation.—Vide Med. Rep. vol. iv. p. 74.

confidently by the professor, have, to say the least of them, always been viewed as a subject of doubtful expediency, particularly in warm countries or low latitudes, where the rapidity of river currents, and the comparatively low range of tides have appeared to present insurmountable obstacles to their successful operation. Common sewers are not invariably calculated for insular situations: the only places where they can be used with effect, are such as are elevated, or at least rendered so twice in twenty-four hours by the recession of tide water. The ordinary tides at this city seldom rise higher than six or seven feet, which does not admit declivity enough to permit them to empty themselves at each reflux: the situation of many of the great cities of Europe, where they are generally introduced, presents no such objection. But, however necessary they might be presumed to be, for the purposes of carrying off all waste and offensive materials in a fluid form, we have no hesitation in reprobating one great use which the author would make of them under any circumstances, and we hope to be long preserved from their benefits.

We cease to be surprised at the Doctor's results, whenever we stop to consider his data. For instance, he says—

"At present, it is calculated that nearly one-twelfth part of the surface of the city is occupied by privies; and consequently, the soil, consisting of a very porous sand, a constant percolation from those deposits of filth into the wells of the city, must be result.—This accounts for the impurity of the water drunk by the inhabitants of New-York, and the effects it produces upon strangers, before they become habituated to its use."—p. 41.

The extravagance of this calculation may have served to introduce the quotation from Volney, equally extraordinary; but we are much deceived if there is a lad in the city, who understands the simplest rule of proportion, who would not detect its fallacy.

Grave-yards, piers, and narrow streets, are the next subjects of remark; and the impropriety of permitting the first to exist within the city is seriously maintained, and the preference given to public and private vaults, contrary to the heretofore received opinion, as interpreted by former Boards of Health. We are now required to believe, that they are very prejudicial, by reason of the emanations from, and their filtrations through, the soil, which, Mr. Volney says, after three days impart to the water of our pumps a *cadaverous* smell! notwithstanding, in the 25th page it is triumphantly objected to the advocate of domestic origin of fever, that they are proverbially healthful. Perhaps it may be as well to transcribe the passages referred to, for the benefit of the author, who, though he may never read his own work again, may notice these remarks.

"Another fact, that cannot fail to silence the sophist on this hackneyed theme, and show the absurdity of resorting to putrid materials, buried beneath the surface of the earth, as the source of our fevers, is the proverbial healthiness of all those parts of the city that are in the immediate vicinity of those churchyards that are still daily employed as places of interment.

"In those enclosures, it is familiarly known that the soil is in a very loose, porous condition; that the bodies interred in it are deposited but a few feet beneath the surface, and consequently, that the air extricated from their decomposition is readily mixed with the external atmosphere. If, therefore, such decomposition of animal matter beneath the surface, could give origin to fevers, I call upon the advocates of this absurd doctrine to assign the reason, not only for the exemption from malignant fevers, but the health enjoyed in the vicinity of those enclosures, and indeed by those who are immediately concerned in the interment of the dead."—pp. 25, 26.

The last subject which the professor recommends to the public authorities, is the introduction of pure and wholesome water, which continues to be a desideratum in this city, and will never cease to be an object of solicitude till it shall be accomplished. It appeared to be almost the

only topic of conversation in the years 1798 and 1799, and claimed precedence of all other preventive measures recommended to the Corporation to secure the city from the visitations of yellow fever. It was in a particular manner owing to the excitement which the various discussions of those measures at that time produced, that the Manhattan Company was instituted with a perpetual charter and banking privileges: public men seemed then to be governed by one sentiment on this subject, and public opinion was directed, as now, to the Bronx and Saw-mill rivers as the sources of supply. The view taken of it in this pamphlet, in so far as it is either important or feasible, will be found among the proceedings of the Common Council of that day, and particularly in the memoirs of Dr. Joseph Browne, and Mr. William Weston, both printed by their order and at their expense;* the plan, as copied, and the manner of treating it, though not very minute and scientific, is still very good, and it is hoped that it will have the effect of directing the attention of that honourable board more to the perusal of their our minutes.

We cannot dismiss this extraordinary performance of the Professor of the Practice of Physic in the University of the State of New-York, without devoting a remark or two on its peculiar character. It is apparent that the author, in making it, had two very different objects in view, both totally distinct from a desire to impart information to his class. His experience of the last summer presented some incidents calculated to affect him unpleasantly, if not to produce some feelings of irritation; and in the first part of his work, in order to show his contempt for the opinions of his medical brethren, he evinces his willingness to cut all communion with them: while, to prove his high respect for "the powers that be," he very respectfully hints, that he would be very happy to continue for some time longer, at

* It was for this reason, perhaps, that the corporation, though they thought it adviseable to request this pamphlet for publication, deemed it inexpedient to pay its expense, there being still many copies of those works on hand.

least, their humble servant. In one of those points, he has laboured hard, and probably successfully, as will be obvious from the whole tenor of the first thirty-one pages: in the other, though he may not succeed, the object is fully manifest from an "unavoidable inference," which we will transcribe.

"It is an unavoidable inference, from the view taken of the importation of fever, that nothing short of the most rigid system of Quarantine Laws, and those too, executed by officers who conscientiously believe in their utility, will secure our cities from a repetition of the evils we have experienced."—p. 23.

What either of these subjects has to do with an introductory lecture, we are at a loss to conjecture; though, we confess, we should not have questioned his right to introduce them, provided he had prosecuted his end, without outraging all respect for his station as a public teacher, by an attempt to stigmatize, as unworthy public confidence or regard, at least nineteen-twentieths of the medical profession. We have little apprehension that the exhibition will ever be repeated, otherwise we should recommend it to the notice of the trustees of the institution, in which he claims to be senior professor, in the fullest confidence that they would prevent it; for miserably defective indeed must be the organization of any academic institution which, whilst it can confer all the rights, honours, and immunities, which appertain to a professor's chair, possesses none of those powers which shall prevent their prostitution.

We were not a little surprised, that the honourable the corporation of our city should have put their *imprimatur* on this paper, by requesting its publication, seeing that by this measure they give it a currency which it would not otherwise possess; and by impliedly sanctioning the doctrines it contains, virtually themselves become a party in this, at present, very unprofitable and unnecessary controversy. But having informed ourselves of the circumstan-

ces attending this extraordinary procedure, we take great pleasure in making known to our readers, that they acted under a persuasion that they were doing a service to the public: besides, but three of their number were present when it was delivered; and as the whole Board were politely invited to attend, they might have been courteous enough to reciprocate the civility by passing the resolution expressive of their wish to see it published.

It is to be regretted, however, that they (in which number are included all the members of the Board of Health, except the three Commissioners of the Health Office,) should have passed the resolution, putting their seal of approbation to this offensive lecture. In their official report of 1819, the confidence of the citizens was claimed by them, on the ground, that they were not the partizans of any particular set of opinions in relation to this question; and their measures heretofore have been marked with a discretion, an energy, and an impartiality, which challenged the respect of every member of the community; but in this case they have unadvisedly done what is proved to have been as unnecessary as it is injurious, (for the lecture was in press before the date of their vote,*) by giving currency to a libel confessedly calculated for electioneering purposes, and to serve the most unhallowed private ends, while they believed that they were benefiting the great interests of the public.

On the whole, if we were asked our opinion of the merits of this lecture, we should be much at a loss for a reply. It appears to us to be a string of opinions, collated from various authors, without any reference to unity of design; it bears upon its face the evidence, that it was not so much intended for physicians as the public generally; and though it is certain, from the scope of the book, that the author is

* The lecture was delivered Nov. 6th, the resolution of the Common Council passed Nov. 27th, the book was published Dec. 4th, and the proof that it *waited* for its imprimatur, will be found recorded on the *blank* leaves accompanying the title-page.

a contagionist, it is extremely difficult to know how to qualify that term respecting him. The whole performance is specious without solidity, and imposing without interest; the confident manner in which it is written, is much better calculated to rivet the prejudices of the ignorant, than to clear up the doubts of the intelligent inquirer; and the unchastened vanity of an author, who, notwithstanding repeated disappointment, still pants after the distinction of striking out some new pathway in science, though at the risk of teaching absurdity, is more apparent than any point which he has endeavoured to establish. All that part of it which is original, and can claim the privilege of copyright, is so happily circumstanced as not to require its protection: and it has this attribute, at least, if it be not imperishable, it certainly is unalienable—the reputation it wins him he shall wear; and we hereby publicly declare, that if, in future, any author, book-maker, pamphleteer, or dealer in literary small wares, shall presume to pirate on *his* property in this lecture, he shall be adjudged guilty of felony, barely upon the presumption that he must be an incorrigible culprit, who will purloin that which can be of no possible use to himself or any one else.

We will now take leave of this academic non-descript and its author, in the hope that when we next hear from him, if it be not on a subject more interesting, it will at least be in a manner less offensive; for if any thing can exceed the mortification of being obliged to write these remarks, it is the conviction we feel that they are but too well merited.

COLLECTANEA CLINICA.

A case of Dyspepsia, attended with an accumulation and long retention in the Intestinal Canal, of various Substances, taken for the relief of the Disease. By JOSEPH M. SMITH, *M. D.*

IN November, 1816, I was consulted by Miss B. an intelligent lady, aged about forty-one years, on account of a deranged state of her digestive organs, which had been of several years standing. Having no notes of the case at present, I am unable to detail minutely the history of her disease. It is recollected that she complained of a general uneasiness over the whole abdomen; her stomach was dyspeptic, and frequently ejected by eructations a considerable quantity of whitish fluid, her bowels were for the most part regular, but were sometimes constipated, and at other times loose; the alvine discharges were generally crude, and otherwise unnatural. Her constitutional symptoms were such as usually attend a derangement of the chylopoietic organs. She experienced great weakness and pain of the loins, and suffered much from fluor albus. The latter complaint had been of long continuance, and was particularly distressing on account of the quantity and acrid quality of the discharge.

A succession of evacuant, chalybeate, and other remedies, were employed without producing any evident amelioration of her symptoms. Injections, per vaginam, and local ablutions with cold water, were the most efficient means of relieving the leucorhæa. In the summer of 1817 she visited Schoolley's mountain, but deriving no benefit from the waters, she returned to the city, and determined to abandon the use of medicines, and to rest her hopes of recovery upon dietetic means. From that period to the

spring of 1820, her health continued nearly the same; the stomach and bowels still appeared to be the seat of disease, and there was frequently reason to suspect some organic lesion of those viscera. Several months after she had discontinued the use of chalybeate medicines, she informed me, that she had discharged from the bowels a considerable quantity of matter, in which the iron could be clearly discerned. After withdrawing my attendance, she was led by her own reflections to the use of enemata. The evacuations procured in this way were copious, and exhibited an unnatural appearance; but afforded no evidence of an obstinate or protracted obstruction of the bowels.

The subsequent history of this case is abstracted from a communication obligingly furnished me by Dr. Pascalis, who was called in attendance on the 21st of April, 1820.

He states, that her skin had a yellowish tinge, and that she was emaciated, and apparently much debilitated. She traced all her complaints to the stomach and bowels; and he remarks, that her symptoms were singular, and novel to him. She had an uncommonly voracious appetite, and was incessantly tortured with spasms about the precordia and diaphragm. The leucorheal discharge continued acrid and offensive, and had greatly exhausted her. She constantly raised from the stomach an abundant quantity of a thick, white, and sweetish fluid, especially when in a recumbent posture. Her pulse was regular, but small, and rather quick and depressed; the tongue presented a reddish appearance.

Concluding from these phenomena that there was some chronic inflammation, or, at least, a state of high nervous excitement, the Doctor immediately proposed blood-letting, which the patient the more cheerfully consented to, as this was the only remedy that had been untried in a former course of treatment. From this she experienced some relief; and the Doctor observes, that "she consented to take a few remedies, and particular articles of diet, which I devised, in order to ascertain, if possible, the diagnosis of a

case, which, in every view, appeared to me very difficult to define. Wishing to improve the opportunity of administering some means of relief, I even went so far as to try, for a short time, the use of mercury, the excitement from which was checked by warm bathing; but neither from this nor any other medicine could I ever discover any satisfactory results. The bleedings, however, were repeated, and always with more or less benefit, especially in her feelings. At length I determined to make a particular examination of the whitish fluid which she constantly raised from her stomach, as I apprehended there might be an ulceration of that viscus, or a scirrhus or stricture of the pylorus. I readily recognized this fluid to be pure chyme. The digestive functions of my patient were evidently slow, and the bowels subject to costiveness. The idea of an ulceration of the stomach was very soon abandoned; and there was no clue by which I could hope to come at the real nature of a complaint which was characterized by so many strange symptoms."

The patient, from her experience, "very judiciously demonstrated" the inutility or prejudicial effects of all ordinary remedies, such as cathartics, emetics, astringents, and tonics, and also of various articles of diet. Blood-letting, which had frequently relieved her, could no longer be resorted to, as in one instance it produced an alarming lipothymia.

The patient was now left, for the most part, to her own management, until the latter part of July; when yielding to the solicitation of a friend, she took a small quantity of Holland gin, and finding some relief, she was induced to repeat the draught occasionally in moderate doses. Experiencing shortly afterward, whilst at stool, a prickling sensation in the rectum, she examined her evacuations, and was surprised to find in them appearances which indicated the presence of pulverized burnt eggshells, which she had taken four years before, on account of some derangement of her catamenia. About the 10th of August, Dr. Pascalis, sup-

posing she might be affected with some extraordinary species of worms, prescribed Madame Nouffer's remedy for tænia, and consequently she was directed to inspect her alvine discharges. The stools afforded no indication of worms, but exhibited a variety of singular and undigested substances. On the 20th of August, she was seized with a violent attack of dysentery, from which she recovered in nine days, under the usual treatment. From that time to the present day, (Jan. 17, 1821,) or rather since she commenced the use of gin, her ordinary alvine evacuations have been accompanied with substances, unaltered in their nature, which were received into her stomach three and four years before. Of these she has separated from the feculent matter, carefully cleansed, and assembled together, an extraordinary variety, and an enormous quantity.

In this wonderful collection, which is preserved by the lady, as a curious memorial of her sufferings, Dr. Pascalis has distinctly recognized the following articles, viz. several teacupsful of eggshells, in powder; three tea spoonsful of brickdust, of the natural colour, which was formerly taken by the advice of a female friend, as a cure for the jaundice; a great quantity of magnesia, in small friable masses; a black powder, evidently precipitated iron, equal in quantity to that contained in eight ounces of the muriated tincture, a medicine of which the patient had taken largely several years ago; half a pint of mustard-seed, still in their natural state, one-third of them having commenced the process of germination; several small white or brown worms, similar to those found in pulpy or drupaceous fruits; a great quantity of quince-seed, which had been taken as an astringent for leucorhæa; minute globules of revived mercury, which, examined with a lens, clearly exhibit the usual appearance of that metal; many small fragments of bones of ordinary meats, and birds; and, finally, at least a pint bowl full of fibrous, husky, or filamentous vegetable substances, derived from indian corn, chesnuts, &c. of which she confesses to have eaten great quantities. Besides

the above articles, there is a considerable quantity of a sandlike or gritty substance, which cannot be traced to aliments, and which is probably the result of internal or fortuitous combinations. During the progressive disclosure of the above phenomena, the health of the patient has very considerably improved. She attributes the removal of the extraneous matters entirely to the use of gin, and artificial warmth applied to the region of the stomach; and in corroboration of this opinion, she asserts, that none of them are found in the fæces if she uses any kind of drastic or cathartic medicines." It is proper to add, that the habits of the patient are uniformly and rigidly temperate.

In respect to this case, we have to remark, that from its commencement its symptoms have been singular, and its nature could never be satisfactorily ascertained. In the course of her disease, the patient has received the advice of several physicians, and taken a great variety of medicines, without any permanent benefit. The occurrences which have recently taken place, obviously elucidate the character of her complaint. It must now be regarded as having been a case of constipation, or, at least, as complicated, with protracted obstruction of the bowels. By the Germans it would be denominated stypsis; and under this term Dr. Parr observes, that "constipation is relative, and it is inconceivable with how little injury to the general health a long-continued obstruction can be borne. There are well recorded instances of its having continued many weeks, and scybala, as well as other substances, have been retained for many years." Cases of this sort might here be enumerated; but we forbear to dilate on this subject, as they must occur to the recollection of every physician acquainted with the records of medicine. We know of no symptoms which can be considered as certainly diagnostic of this form of disease. Complaints of the stomach and bowels are frequently so anomalous, as to render it difficult, if not impossible, to form a correct opinion of their nature. We may observe, however, that attention to the appear-

ance and frequency of the alvine discharges, as recommended by Mr. Abernethy, in his observations on the constitutional origin and treatment of local diseases, will, in general, enable us to proceed judiciously.

A Case of Laryngo-Tracheotomy, successfully performed by A. R. P. DUCHATEAU, *Military Surgeon of the Hospital of Arras, &c.*

ON the evening of the 23d November, 1815, Iphigenia Ansart, aged six and a half years, and of a lymphatic temperament, swallowed, whilst playing with her companions, a plum-drupe. I was immediately sent for, and found her labouring under inexpressible anxiety, with the voice altered, and hissing. The slight cough which attended the accident in the first moments was allayed, but at each strong expiration, the noise of a foreign body striking against the glottis could be heard; she referred, as the seat of her distress, to the superior and lateral part of the thorax; all which indicated the presence of the drupe in one of the bronchiæ. Dr. Leviez and myself agreed to administer ten grains of ipecacuanha, which produced five or six ejections, without the desired result. The child was now greatly fatigued, and fell asleep; in which state she continued very tranquil until near four o'clock in the morning. On awaking she became agitated, convulsed, and extremely feeble. At seven o'clock we found her unable to articulate, and without sense. The eyes were closed, the face pale, the pulse small, and the respiration slow and laborious. We did not conceal from the parents her hazardous situation, and the approaching event, unless the offending substance was removed by an operation. They yielded her wholly to our care, and left the house. We placed her on a mattress, with the head elevated on a pillow, and after drawing aside a fold of the integuments,

the anterior and inferior part of the larynx, and the superior and anterior part of the trachea, were laid bare by a longitudinal incision of an inch and a half long. The laryngeal and tracheal cavities were then opened, by passing the bistoury into the crico-thyroid ligament, and dividing the cricoid cartilage and three of the tracheal rings. Immediately on penetrating into the larynx, the air rushed out with force, and the drupe struck against the point of the knife. After waiting a short time, it again presented at the opening; but could not be expelled, owing to the feebleness of the expiration. I then endeavoured to seize it with the forceps, but was foiled in the attempt, and afterwards searched for it in the larynx with a sound; but found it had refallen into the trachea, where it produced alarming suffocation, and the child appeared on the point of expiring. Whilst deliberating on the steps to be taken under these circumstances, an irritation excited in the nose, produced violent action of the respiratory organs, and the offending body was thrown out by a strong expiration, and bounded on the floor. The child immediately revived, and attempted to speak.—No artery had been divided in the operation. The lips of the wound were brought together by adhesive straps, and the ordinary dressings applied. A slight cough continued for a few days, the wound was completely cicatrized in two weeks, and the child perfectly recovered, without any change of voice, or other inconvenience, and has since enjoyed good health.—The dimensions of the drupe were, in length, nine lines, in breadth four and a half, and in thickness two and a half.

(Nouveau Journal de Médecine.)

INTELLIGENCE.

A Circular Address to the Officers composing the Medical Staff, by Samuel L. Mitchill, M. D. & P. Surgeon General of the Militia in the State of New-York.

GENTLEMEN,

DURING a season of peace and prosperity, when every citizen is occupied in his individual pursuits, more than in guarding against public danger, there appears a peculiar propriety in presenting to you a few subjects for consideration.

Although, at present, there is no field for the great operations of Military Surgery, the Surgeon General believes the talents of the Surgeons attached to the Hospital Department and to regiments, and those of their respective assistants, may be constantly and worthily employed, in promoting the great objects for which they are commissioned, and in contributing to the advancement of the profession at large.

We live in a section of the globe, where, at all times, diseases and painful accidents are frequent, and where the aid of professional men is sought with great solicitude. The deference thus paid to the opinion and character of Doctors, places them on elevated ground. By common consent, they are the guides and directors of health. To the honour of our country it has been remarked, that where to intelligence and activity, there is united a correct morality, a suffering individual and his family rest upon their Physician with perfect confidence; and well they may. A seat in the House and in the Senate; high commands in the Army and Navy; and the dignity of Governor in the States, has frequently been conferred, by the voice of the

people, the legitimate source of authority, upon medical men.

The public sentiment in favour of the faculty, has been strongly expressed in as many places, as there are schools of medicine. It is right, in our condition of society, to adopt the measures necessary for forming able Physicians and Surgeons. No person is exempt from the casualties that may render their help important. All rational creatures pray that the healing powers may be propitious. So they ought.

Distempers often assail with vehemence, domestic and settled life. More severe and formidable are their attacks upon the exposed, and sometimes unsheltered soldier. Both kinds of malady require your particular study. During the absence of tumult and war, you can improve the opportunities afforded for observation. You can watch symptoms with calmness and care. There are none of the marchings nor counter-marchings of a campaign to interrupt you.

In this posture of things, I recommend to you, the observance of EPIDEMIC DISEASES. They come and they go, in a way that is not yet well understood. It ought to be comprehended, if possible, how far they are connected with solar influence, terrestrial effluvia, atmospheric change, corporeal predisposition, or other causes. Your opportunities are excellent. You traverse all the inhabited parts of the commonwealth. The commencement, progress, and termination of such maladies, are open to your view. You know into what they degenerate, when wholly neglected or unskilfully treated.

That my meaning may be comprehended, I offer a few examples.

Our *Winter Fevers* may possibly visit us again. Though they were skilfully described, among other writers, by North, Gallup, and Wilson, their reappearance deserves the utmost vigilance. It is not their mortality alone that deserves attention. Their obscure origin, when snow and

ice lock up the miasmata of marshes, and where the sequestered abodes of the sick forbid the entrance of contagion, ought to be illuminated by the torch of science. It is deemed essential to trace the operation of animal food, and of ardent spirits, upon constitutions thus invaded.

Should the *Pneumonia Typhoides*, so fatal to our armies during the late war, break out once more, every memorable fact concerning it ought to be recorded. A disease attacking the vital organs with so much fury, merits the most particular notice. There will be no small satisfaction in discovering the morbid predisposition and cause; in ascertaining the injury done to the bodily organs, and the value or inefficacy of prescriptions. The writings of Mann, Heustis, and Waterhouse, will afford you encouragement and direction on these and similar inquiries.

The *Influenza*, or *General Catarrh*, has had an extensive prevalence during the present autumn. Additions are wanted to complete its rational history. The prevalence of such wide-spreading disorders, their forerunners, their concomitants, and their consequences, are all of high moment to medicine and to mankind.

Dysenteries merit the most particular attention. Connected with the food and drink received into the alimentary canal, they rage with a distressful, and frequently, a destructive sway. The amount of disorder in the stomach and intestines; the degree and type of the fever; the connexion of both with the air respired, invite the most careful inquiry; as also do the circumstances relative to their origin and communication.

Who are more capable than you, to observe every thing that relates to the *Remitting and Intermitting Fevers*, which almost every season prevail to some degree and extent over certain districts? Whether they assume the solemn aspect of the Bilious, Malignant, Autumnal, or Yellow, or the milder complexion of Quotidian, Tertian, or Quartan; there are circumstances that often solicit the regard of the medical philosopher. It must be owned, we are at

this moment in want of a satisfactory theory of Fever, notwithstanding the labours of Boerhaave, Cullen, Darwin, and Rush. You are called upon to furnish information in this exigency. Perhaps from some quarter whence it is least expected, the long-sought light may shine. I should exult with a joy greater than I can express, if, in my day, the phenomena upon this momentous, but abstruse subject, could be generalized into a science.

It would be useful to make correct entries in a book, of all the memorable events, medical, surgical, or collateral, that come within your observation. From this they may be copied from time to time, and forwarded for insertion in one of the respectable journals published quarter-yearly in New-York, Boston, and Philadelphia.

It is suggested as a steady rule of conduct, in all important cases, to inquire, by actual inspection, into the morbid alterations wrought by disease.

The situation and employment of many of you is favourable to a knowledge of our indigenous simples, and an acquaintance with their virtues. The forests and fields are rich with vegetable productions of almost every sort. The Physician, as he rides through the woods, beholds sanative plants on the right and the left; and the Surgeon, as he journeys along the highway, may be said to pass through an avenue of vulnerary herbs. In ascertaining their names, localities, and uses, you will not only act worthily for yourselves and your patients, but advantageously for the whole profession, and the human race.

The residence of some of those whom I address, is favourable to a more minute acquaintance with the distemper in rabid brutes, which, by their bites, excites hydrophobia in man. Your attention is particularly invited to mad foxes and mad wolves, as well as to mad dogs. The gazettes, for several years, have contained shocking recitals of attacks made in their paroxysms of fury, upon man and domestic animals. It is hoped we shall obtain an embodied his-

tory of those mournful occurrences, written at full length by competent members of the staff, instead of being left to the authors of casual paragraphs, soon to be forgotten and lost.

From men so happily located as you are, we naturally expect intelligence concerning the topography of the country. After the excellent descriptions of New-York County, by Akerly; of Jefferson County, by Henderson; and of Saratoga, by Steel; more may be expected. Would the sons of Æsculapius really perform as much as they are capacitated to achieve, a shower of information would descend upon us. For such exercises, yourselves are better qualified, by reason of your scientific education, and your enlarged opportunities for survey, than any other persons. The former prepares you to understand, and the latter enables you to arrange and narrate. In examining the soil you tread, you will not fail to note the composition of the strata, of the issuing waters, and of the incumbent air. I exhort you to collect and preserve those monuments of the former generations of created beings, the remains of antediluvian vegetables and animals, which are distributed in many places through the ground and the rocks.

It cannot be considered foreign to my purpose, to invite your attention to the effects of certain deleterious agents upon ourselves and other creatures. The poisonous operation of the Sumachs, for example, by their effluvia and juices, has not been thoroughly investigated. The slabbering disease of horses, deserves a more profound inquiry than has been bestowed upon it. If it arises from the acrid juice of succulent plants, in the pasture, it becomes us to know, by a series of well-conducted experiments and observations, what those productions are. When detected, it may, perhaps, be possible to eradicate them from the meadows. The facts concerning the death of oxen, after eating the leaves and twigs of the wild cherry-tree, ought to be properly stated.

The effects of the venom of the rattle-snake and of the black adder, deserve a more minute and careful inquiry, from the citizens of a country infested by those serpents. Their history, their seasons of greatest virulence, and an account of the best remedies among the great number recommended, ought to be given in a form worthy of the age, and the society in which we live.

Spurred rye has become a fashionable remedy. Yet injurious effects are ascribed to ergoted bread-corn, when used in diet, by the inhabitants of France and of some other parts of Europe. Its action upon the health of our citizens ought to be fully investigated, and to you I commit the inquiry. Such diseased grain, or a fungus sprouting up in the place of grain, is at best suspicious. There is no doubt, in my mind, that all the spurred rye and wheat, ought to be separated from the grist, before it is carried to the mill. But if careless persons omit this precaution, and eat bread containing such an improper ingredient, the operation ought to be traced by Physicians. Its alleged noxious action upon domestic animals, cannot fail to attract your attention.

For the present, I shall content myself with the hints contained in the preceding paragraphs; while I request that your liberality and patriotism may receive them in the same spirit that they are written. You are young, with the world of business and usefulness in broad display before you. Act as becomes yourselves and your stations, and you will receive the reward due to excellent deeds.

I congratulate you on the completion of the National Pharmacopœia, which may be shortly expected from the press.

Every order to me from the Commander in Chief, touching the Medical Staff, shall be immediately communicated to you.

I beg you to remember, that on all occasions, in peace and war, I shall labour to promote our country's welfare.

and to discern and appreciate merit in the individuals belonging to my department.

State of New-York, Nov. 18, 1820.

On the Fascia Iliaca. *By* A. H. Stevens, M. D.

The medical public in this country has been much interested during the last and present years, in the discussions that have taken place respecting the anatomy of the bladder, in reference particularly to the operation of lithotomy. Several very minute descriptions of this organ, and its connexions, have been recently given by foreign authors, and by writers in the Medical Recorder, published at Philadelphia; and although some of these statements appear to be inaccurate, and others to afford imperfect views of the relations of this viscus, too much credit cannot be given to the very distinguished anatomist who has directed the attention of surgeons to this important subject.

The design of the present paper is to communicate the result of several dissections, made by my friend, Mr. William Anderson, Surgeon, of Nova-Scotia, and myself, in order to ascertain the origin, connexions, and extent of the membranous partition between the pelvis and the perineum. For this purpose, we dissected from the perineum its superficial fascia, removed the adipose substance, and with the handle of the scalpel, separated the levator ani muscle from the fascia covering the obturator internus. If there were any fascia continuous with that covering the obturator internus muscle inserted into the base of the prostate,*

* The prostate fascia, when superficially looked at, would appear to take its origin from the inner rami of the ischium and pubis. If, however, we come to examine it more attentively, we may remark, that although it has here a connexion with the bone, that it does not terminate here, but that in fact it is continuous with the aponeurosis, which covers the obturator inter-

we judged that it might be demonstrated by this method of proceeding. But the possibility of such a structure seemed to be irreconcileable with the origination of the levator ani, from the internal surface of the ischium and pubis, as described by John Bell, Wistar, and other anatomical writers.* Satisfied, therefore, that some inaccuracy had crept into this part of the description of the prostate fascia, we turned our attention to the prostate gland. This was seen to be invested with a fascia, in all that part of its surface which is not in contact with the bladder.†

The levator ani muscle was now removed, and thus a fuller view was obtained of this fascia, running on each side from the medial line of the prostate, in a direction upward, backward, and outward, in contact with the neck of the bladder, until it almost reached that part of this viscus which is opposite to the superior margin of the foramen thyroideum. It was then seen to leave the sides of the bladder, to insert itself into the brim of the pelvis.‡

nus muscle. The best description, perhaps, that could be given of it, would be to state, that the fascia which covers the obturator internus muscle, having reached the rami of the ischium and pubes, forms there a connexion with the inner margins of these processes, and that from this it runs down, and is at last lost by becoming inserted into the basis of the prostate gland, and into the rectum, where it lies below and exterior to the gland. *Mr. Pattison's Paper in the Medical Recorder, vol. iii p.* 11.

* It [the levator ani] continues its origin from the internal surface of the pubis, all the way to the sacrum. It grows gradually smaller, as it goes downwards to surround the anus. So it is inserted into the point of the os coccygis, and it is mixed with the spincter ani muscle. The whole pelvis is lined with it like a funnel or inverted cone. The wider part representing its origin from the pelvis; the narrower part, its insertion into the anus. The whole bladder is surrounded and covered with this muscle. *Bell's Anatomy.*

The levator ani arises from the os pubis, within the pelvis, as far up as the edge of the foramen thyroideum, and the joining of the os pubis and os ischium, forms the thin membrane that covers the obturator internus, and coccygeus muscle, from the spinous process of the os ischium. *Wistar's Anatomy.*

† This fascia must be cut by every incision into the prostate gland.

‡ This probably is the fascia described by Professor Pattison. The place of its attachment to the bladder, and the slight degree of obliquity of that attachment to the incision of the prostate in the lateral operation of litho-

Here then was the *perineal* side of the partition between the perineum and the pelvis.

The peritoneum having been dissected from the iliac fascia, the bladder was next separated from the sides of the pelvis, and some interposed adipose substance removed. The fascia iliaca was thus seen to extend from the linea ilio-pectinea downwards, in contact with the sides of the pelvis, about half way to the superior margin of the foramen thyroideum; or, to state it more particularly, until it reached an imaginary semi-elliptical line over the foramen thyroideum, the middle of which should be equi-distant from that foramen and the linea ilio-pectinea. Along this line the iliac fascia leaves the walls of the pelvis, and passing towards the bladder, it forms the *pelvic* side of the partition, between the pelvis and the perineum, and having reached that part of the viscus, which is a little below the centre of the foramen thyroideum, it is reflected upwards, and is lost upon the fundus.

The bladder having been moderately inflated with air, was drawn a little to one side, and by placing the parts before a strong light, a transparent membrane was beautifully displayed, consisting of the first-described fascia, coming up from the prostate and inferior surface of the bladder; and of the last-described extension of the iliac fascia; formed, in fact, by the two lamina of the iliac fascia before they separate to cover respectively the inferior and superior surface of that viscus. This fascia resembles, in shape, the superficies of a longitudinal section of an egg, but is rather smaller. Its attachment to the sides of the pelvis is such as I have described, and it applies itself to the bladder along a line drawn somewhat above the base of the prostate, in a direction backwards and upwards.

In its course from the sides of the pelvis to the bladder, it passes obliquely inward, downward, and forward.

It is scarcely to be expected, that mere description

tomy, would seem to place it entirely out of the reach of the gorget, or knife employed according to the rules commonly laid down.

should convey clear ideas of a structure so complicated as that under consideration. It is believed, however, that the above account will enable any one, moderately versed in anatomy, to dissect the parts in such a way as to satisfy himself of the correctness of what has been stated.

The importance of an exact knowledge of the partition between the pelvis and the perineum, in the operation of puncturing the bladder per perineum.* and in accidental injuries of these parts, and the adaptation of the lowest part of the iliac fascia to the several purposes of supporting, allowing of a rising of that viscus when distended, and finally of limiting the extent of its rising, are too obvious to escape notice.

The relaxation of the levator ani by the descent of this extension of the iliac fascia, with which it is closely connected, and from which it would appear, indeed, to receive its origin, may perhaps explain, in some degree, the facility with which the fæces are evacuated after urination, and the impossibility of emptying simultaneously the bladder and the rectum at the same time.

New-York, Jan. 1821.

Vegetable Thunder-Rods.

From some ingenious experiments, made by Lapostolle, Apothecary at Amiens, it appears that straw is as perfect a conductor of electricity as any of the metals. With the end of a cord of this material an inch long, the electric fluid may be drawn from the strongest battery, without experiencing the slightest shock. The discoverer thinks the

* In puncturing the bladder per perineum, this partition between the pelvis and the perineum would be drawn more out of the reach of the trocar, if the anus were at the same time pushed up which probably would also tend to remove the pouch of the rectum from the point of the trocar, and the edge of the gorget in the operation of Lithotomy.

country may be sheltered from the ravages of thunder, and even of hail, by elevating on every sixty acres a pole twenty feet high, with a straw cord, surmounted by a metallic point.

(*Jour. Universel des Scien. Med.*)

An Account of the Births, Marriages, and Deaths, in Paris, during the year 1819.

MARRIAGES				6246
BIRTHS	Males	12403		24342
	Females	11939		
ILLEGITIMATE CHILDREN	acknowledged	1984		8641
	not acknowledged	6657		
DECEASED	at their residence: Males	10768	22344	22670
	at their residence: Females	11576		
	by chance-medley: Males	286	326	
	by chance-medley: Females	40		
STILLBORN	Males	757		1352
	Females	596		
SUICIDES	From disappointment in love		25	376
	From disgust of life, mental alienation, and domestic troubles		116	
	From dissipation, gambling, debauchery		52	
	From poverty, loss of occupation, or derangement of affairs		93	
	From fear of reproach or punishment		12	
	From unknown motives		78	

(*Journal Universel des Sciences Médicales.*)

Description of two mammiferous Animals of North America. By Samuel L. Mitchill, M. D. Professor, &c.

(Read before the Lyceum of Natural History.)

The Federation Squirrel *of the region situated near the sources of the River Mississippi.*

Sciurus *tridecem-lineatus; juxta fontes fluvii Mississippi habitans.*

The thirteen stripes along the back of this superb little quadruped, place him in a remarkable association with the thirteen states of the North American confederacy, and with the thirteen stripes and thirteen stars of their flag.—It is about the size of the ground squirrel, or Sciurus Striatus. The length rather less than twelve inches; measuring from the nose to the extremity of the tail. The tail itself is about three inches long.

There are four claws on the fore-feet, and five on the hinder.—Body slender; head taper; nose pointed. The most striking and remarkable character of this animal, is his variegated back. Of this the colour is a sorrel, or dark chesnut, which extends down the sides. Through this run, longitudinally, whitish stripes or lines, and rows of whitish spots alternately. At the nape of the neck commences the middle line, which is continued for more than half its length, whence it becomes interrupted with spots until it reaches the tail, where it terminates. On each side of this line, which is parallel to the spine of the back, are three continued lines, and three dotted lines, reaching from the head to the rump. The two immediately next the middle line, one on each side, are continued lines. The two next to these, one on each side, are dotted lines, or rows of spots; there being twenty spots in each that can be distinctly counted; and so on by turns, until the whole thirteen are displayed.

The colour of the belly, breast, and throat, is pale yel-

low, or yellowish white. The upper side of the tail is diversified with spots; while the under side, and the ends of the hairs around, are of the same complexion with the belly.

This creature is by far the most beautiful of the whole squirrel family; surpassing by many degrees the elegant *Sciurus Striatus*, or ground squirrel. The striped squirrel is called *ground squirrel*, because he makes his habitation in the ground, (habitat sub terra.)

Brought by Professor Douglas, of the United States Military Academy, from the region bordering the sources of the river Mississippi, in November, 1820.

The Gopher, or Pouched Rat of North America, (*Mus bursarius.*)

MUS SACCATUS, *juxta regionem circa Lacum Superiorem sitam.*

Doubts having been entertained, (Cuvier Regne Animal, vol. i. p. 199,) whether this animal, announced to zoologists by Shaw, was sufficiently and credibly established, I have the pleasure to assure naturalists, that a complete dried preparation, brought by Captain Douglas, Professor, &c. is now in my possession. It is undoubtedly the real *mus bursarius* of our continent. As it has been already introduced to the learned world, I shall content myself with noting some of its most remarkable characters.

Length about eleven inches, from the nose to the rump. In my specimen there is no tail, nor can a vestige be found. If a tail had existed, it was effectually destroyed and effaced.

The fore-feet are furnished with five claws, three of which are long and strong, as if intended for digging and burrowing. They are curved, acuminated, and semi-transparent; the middle one being nearly an inch long.

They have some resemblance to the paws of the mole, (talpa,) though they are not palmated in the same manner. The hind-feet are comparatively small and diminutive, resembling those of the common rat, and armed, each also with five claws: the whole space from the heel forwards being a sole of the foot, or a walking surface.

Body thick; head obtuse; nose blunt;—the most striking and remarkable character, is the pouched neck. There are two sacs, or pouches, jutting when distended, an inch or more from each side of the neck. The circumference of either, near the neck, is more than two inches. These pouches are covered with very short hair, finer and paler than that of the body. They give it a very peculiar and grotesque appearance; for the distance from the extremity of one pouch to the extremity of the other, measuring straight across the neck, is four inches and one quarter.

The upper teeth are grooved or sulcated on the outside. Each of the two teeth has a double furrow from the gum to the point, lengthways; one furrow passing along the middle of the tooth, and being deep, while the other passes near the inner edge or side, and is shallow. Hair very much like that of the mus decumanus, or common rat, but shorter, finer, and more uniformly brown on the belly, as well as on the back.

Brought from the region bordering on Lake Superior.

Clinical Hospital.

We are gratified to learn, that the Inspectors of the State Prison in this City, have acted with becoming liberality towards our College of Physicians and Surgeons. Upon application being made by that learned and respectable body, for the facilities of clinical instruction, which the hospital connected with the prison affords, the Inspectors

immediately passed a resolution, importing their acquiescence in the request of the College. The medical students have now access to regular clinical wards, where their professor receives every accommodation for the regular execution of his collegiate duties.

The medical department of this institution now consists of two physicians and one surgeon—to wit:

William Hammersley, M. D. C. Prof. } Physicians,
Joseph M. Smith, M. D. }
Charles Drake, M. D. Surgeon.

A New Surgical Work.

To be published in quarto, in the course of next spring, The Surgical Anatomy of the Groin and Pelvis, as connected with the different species of Hernia; as also, the Operations of Lithotomy, and for Tying the External Iliac Artery. By *William Anderson*, Esq. Licentiate of the Royal College of Surgeons of Edinburgh.

N. B. This Work is intended as the first part of a course of the General Surgical Anatomy of the Human Body. It will be accompanied with about six copperplate engravings, carefully executed, and attended by the most minute anatomical description, together with clear directions for dissecting the parts in the dead subject.

German Correspondent.

This interesting periodical work is principally intended to acquaint the people of these States with the progressive state of literature and science in Germany. When it is considered that similar undertakings have been successfully patronised in that country, for the purpose of noticing and reviewing all scientific American productions, and that the German genius and their literary labours have

largely contributed to the general improvement of useful knowledge, it is hoped that neither materials nor patronage will be wanting for the promotion and support of this work. We have perused the first six numbers, and take great pleasure in recommending it to the public.

CARD.

The following is the replication of Dr. Watts, to the Card of Dr. Neilson, which appeared in our last Number.

In answer to the Card of Dr. Neilson in the last Number of the Medical Repository, I send for insertion the following extracts of letters in my possession. They supersede the necessity of any remarks on the character and origin of the Card in question, and prove beyond doubt, that the cases of disease alluded to, were not "prescribed for and treated under the sole and immediate directions" of Dr. Neilson, as therein stated. Dr. Peter B. Helme, in a letter addressed to me, says, "In answer to your inquiries with regard to the cases of disease supposed to have originated from drinking cold water, which appeared in the New-York Hospital in the summer of 1818, I have conceived it to be my duty to state, that I was present about the time of the reception of several of those patients; that I was in the habit of visiting the Hospital frequently at that time and afterward. I recollect distinctly, that at the time alluded to, Dr. Neilson was the regular attending physician of the house; but that from a difficulty in finding Dr. Neilson at the time, and from the urgency of the cases, it is my impression you were sent for by the house physician, and that you recommended the course of treatment which was adopted with such decided success. I never heard a doubt expressed on this

subject, until the "*Note* of Dr. Neilson in the Medical Repository."

Dr. James M. Pendleton, in a letter to me a few days since, says, I "was house-walker at the time, but absent from town. I received from Dr. Campbell, the house physician, a letter containing all the details on the subject, and the extraordinary success which attended the mode of treatment adopted, and the credit of which he unreservedly gave to yourself, and mentioned your attendance at the time the remedies were administered. After my return to town, these cases were the subject of frequent conversations between ourselves, in which he always mentioned you as having first suggested the treatment, nor was any other person ever even alluded to as having any participation in it."

Dr. Campbell, the house physician referred to in the preceding extract, now of Charleston, South Carolina, writes to me, that "on the 30th June, 1818, about two o'clock, P. M. several patients were brought into the New-York Hospital, evidently apoplectic, and said to be caused by drinking cold water. As was my duty, I sent to inform Dr. Neilson, the physician in attendance; but in order to have more immediate assistance, I sent to you, who were so much nearer. You came immediately, and were for the time of course the physician in attendance, by whom I was to be directed. You observed to me, that it was a disease of the head, and not of the stomach; that bleeding, and not laudanum, was the remedy; and by your advice the temporal arteries, and a vein in the arm, were opened, so as to abstract blood as rapidly as possible. You had them put into a warm bath, and its being ready at the time, saved the life of a little child brought in with convulsions. Sinapisms to the breast, and scrobiculus cordis, were applied by your direction. After you left the hospital, Dr. Neilson came, and resumed his situation; two or three more were brought in, for whom he prescribed bleeding, and of course continued his attendance on the

cases, as well as to witness and approve the examination of the Welchman who died. One case I had the management of entirely in the first instance, as it occurred at the time in which the attending physician was not there, and the house physician you know has considerable discretionary power. Some workmen were engaged in making a coal vault in the hospital yard; one of them, drinking cold water, was immediately seized with apoplexy, and was admitted as a patient. Having so recently witnessed the efficacy of your treatment, I adopted it, and bled him more largely than any of the others; his recovery was rapid, and when Dr. Neilson came in the morning, he hardly considered him a subject for prescription. I believe this was the only patient who had no supervening affection of the chest."

There is here, I think, abundant evidence of the inaccuracy at least of the Card, and also of my having treated many of the first cases of that disease, notwithstanding the exclusive management of it so incautiously assumed by Dr. Neilson.

JOHN WATTS, Jun.

December 28, 1820.

CARD.

One of the Editors dissents from his Colleague on some of the opinions in the review of Dr. Devèze's work on Yellow Fever. He thinks the Doctor may justly complain of the medical writers in this country not acknowledging his claim of priority to the doctrine of the non-contagious character of yellow fever; but at the same time, he would object to one of the motives assigned by the the author for such neglect, to wit; that American physicians are averse from citing foreign authorities. Now if there be a fault on this head, it is certainly the reverse of

this, which has led foreign journals to attribute the circumstance to poverty of intellect. Dr. Rush was as early an opponent to the doctrine of importation as Dr. Devèze, and if he continued to advocate the contagious character of the disease until 1799, he had reason, in his subsequent writings, to consider himself as an independent collaborator, and not called upon, in promulgating an important medical doctrine, to tread in the footsteps of another, when he knew so well how to defend it in his own masterly manner. Besides, his acknowledgment to one person would not have relieved him from the same obligation to many others, who followed during the interval of six years, from 1793 to 1799; among whom were Miller, Mitchill, Smith, Bayley, &c. who were his countrymen. It is also a fact, that the first dissertation of our author was kept for many years from occupying much of the public attention, by many editions of a popular pamphlet on the contagion and importation of yellow fever, published by M. Carey; and the College of Physicians of Philadelphia also, about this time issued, and profusely circulated, their facts and observations in defence of their exclusive doctrine of importation.

☞ The Editors beg leave to apologize for the omission of the Meteorological Tables for the last quarter; which have given place to articles of more immediate interest, and will be inserted in the next number. They propose in future to give accounts of the Atmospheric Constitution and Diseases annually, as better calculated to exhibit correct and more comprehensive views on this important subject.

Spiræa Tomentosa.

Miss Sarah Starr del. A. Anderson Sc.